The SCIENTIFIC MONTHLY

VOL. 80 NO. 4

APRIL 1955

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VOL. 80 APRIL 1955 NO. 4

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THE SCIENTIFIC MONTHLY, established in 1872 as *The Popular Science Monthly*, has been an official publication of the American Association for the Advancement of Science since 1915. It is published for the Association at Business Press, Inc., Lancaster, Pa.

 Entered at the Lancaster, Pa., Post Office as second class matter under the act of March 3, 1879.

All **correspondence** should be addressed to THE SCIENTIFIC MONTHLY, 1515 Massachusetts Ave., N.W., Washington 5, D. C. Manuscripts should be typed with double spacing and submitted in duplicate. The AAAS assumes no responsibility for the safety of manuscripts and photographs or for the statements and opinions expressed by contributors.

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THE SCIENTIFIC MONTHLY is indexed in the *Reader's Guide to Periodical Literature*.

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Meetings

April

27–29. Conf. on Nuclear Engineering, Los Angeles, Calif. (Univ. of California Extension, Los Angeles 24.)

28. West Virginia Acad. of Science, Fairmont, W. Va. (J. T. Handlan, Jr., Potomac State College, Keyser, W. Va.)

29–30. Symposium on Advances in Chelate Chemistry, New York, N. Y. (H. P. Gregor, Polytechnic Inst. of Brooklyn, 99 Livingston St., Brooklyn 1, N. Y.)

30. New Hampshire Acad. of Science, Hanover, N. H. (H. W. Curtis, Dept. of Elec. Engineering Dartmouth College, Hanover.)

May

1–3. American Assoc. of Colleges of Pharmacy, Miami, Fla. (R. A. Deno, College of Pharmacy, Univ. of Michigan, Ann Arbor.)

1–3. American Soc. of Hospital Pharmacists, Miami Beach, Fla. (G. Neimeyer, 2215 Constitution Ave., NW, Washington, D. C.)

1–4. American Institute of Chemical Enginers, Houston, Texas. (S. L. Tyler, AICE, 120 E. 41 St., New York 17.)

2–4. American Geophysical Union, annual, Washington, D. C. (W. E. Smith, AGU, 1530 P St., NW, Washington 5.)

2–5. Electrochemical Soc., Cincinnati, Ohio. (H. B. Linford, 216 W. 102 St., New York 25.)

2–5. Research Equipment Exhibit and Symposium in Instrument Techniques and Applications, annual, National Institutes of Health, Bethesda, Md. (Supply Management Branch, NIH, Bethesda 14.)

4–5 American Psychosomatic Soc., 12th annual, Atlantic City, N. J. (L. S. Kubie, APS, 551 Madison Ave., New York 22.)

4–6. American Inst. of Electrical Engineers (Middle Eastern District), Columbus, Ohio. (N. S. Hibshem, AIEE, 33 W. 39 St., New York 18.)

4–6. American Meteorological Soc., Washington, D.C. (K. C. Spengler, A. S., 3 Joy St., Boston 8, Mass.)

5–7. American Ethnological Soc., Bloomington, Ind. Alice G. James, Hunter College, 695 Park Ave., New York 21.)

5–7 Illinois State Acad. of Science, Carbondale. (L. E. Bamber, 223 Natural History Bldg., Urbana, Ill.)

5–7. Kansas Acad. of Science, Lawrence. (C. T. Rogerson, Dept. of Botany, Kansas State College, Manhattan.)

5–7 Soc. for American Archaeology, Bloomington, Ind. (G. I. Quimby, Chicago Natural History Museum, Chicago 5, Ill.)

5–7. Soc. for Applied Anthropology, Bloomington, Ind. (E. Purcell, SAA, 61 W. 55 St., New York 19.)

6. American Assoc. of Spectrographers, 6th annual, Chicago, Ill. (F. E. Stedman, Engineering Research Laboratory, Bendix Aviation Corp., 401 N. Bendix Dr., South Bend 20, Ind.)

6. Conference for Engineers, 2nd annual, Columbus, Ohio. (H. A. Bolz, College of Engineering, Ohio State Univ., Columbus.)

6–7. Illinois Acad. of Science, annual, Carbondale, (L. E. Bamber, 233 Natural History, Univ. of Illinois, Urbana.)

6–7. North Carolina Acad. of Science, annual, Davidson. (J. A. Yarbrough, Meredith College, Raleigh, N. C.)

6–7. North Dakota Acad. of Science, annual, Grand Forks. (J. D. Henderson, University Station, Grand Forks.)

6–7. South Dakota Acad. of Science, Brookings, (A. L. Haines, Chemistry Dept., Univ. of South Dakota, Vermillion.)

6–8. American Psychoanalytic Assoc., Atlantic City, N. J. (R. L. Frank, 745 5 Ave., New York 22.)

8–12. Soc. of American Bacteriologists, annual, New York. (J. H. Bailey, Sterling Winthrop Research Inst., Rensselaer, N. Y.)

9–13. American Psychiatric Assoc., 111th annual, Atlantic City, N. J. (W. Malamud, 80 E. Concord St., Boston 18, Mass.)

10. World Health Assembly, 8th, Mexico City, Mexico. (World Health Organization, Palais des Nations, Geneva.)

12–14. American Assoc. of the History of Medicine, 28th annual, Detroit, Mich. (E. H. Ackerknecht, School of Medicine, Univ. of Wisconsin, Madison 6.)

12–14 Virginia Acad. of Science, Harrisonburg, (F. F. Smith, P. O. Box 1420, Richmond, Va.)

12–19. Latin-American Cong. of Chemistry, Caracas, Venezuela. (Jose L. Prado, Edificio Industria-Puente Republica, Apartado de Correos No. 3895, Caracas.)

13–14. American Assoc. for Cleft Palate Rehabilitation, 13th annual, Boston, Mass. (A. Fox, 1653 Main St., Springfield, Mass.)

13–14. American Physical Soc., New York State Sec. Buffalo. (L. W. Phillips, Univ. of Buffalo, Buffalo 14.)

14–21. European Federation for Chemical Engineering, Frankfurt A.M., Germany. (Dechema, Frankfurt A.M. W. 13.)

16–19. American Assoc. of Cereal Chemists, St. Louis, Mo. (C. L. Brooke, Merck and Co., Inc., Rahway, N. J.)

16–20. National Conf. on Weights and Measures, 40th, Washington, D. C. (W. S. Bussey, National Bureau of Standards, Washington 25.)

18–20. American College of Cardiology, 4th annual, New York, N. Y. (P. Reichert, ACC, 140 W. 57 St., New York 19.)

18–20. European Assoc. of Exploration Geophysicists, 8th meeting, Paris. (Dr. B. Baars, 30 Carel von Bylandtlaan, The Hague.)

19. Maryland Acad. of Science, annual, Baltimore. (J. W. Easter, Mt. Vernon Woodberry Mills, Mercantile Trust Bldg., Baltimore 2.)

19–20. Soc. of Exploration Geophysicists, 10th annual Gulf Coast meeting, San Antonio, Tex. (SEG, 624 S. Cheyenne, Tulsa, Okla.)

23–25. American Trudeau Soc., Milwaukee, Wis. (Natl. Tuberculosis Assoc., 1790 Broadway, New York 19.)

23–26. International Surgical Cong., Geneva. (M. Thorek, 1516 Lake Shore Dr., Chicago, Ill.)

23–27. National Tuberculosis Assoc., annual, Milwaukee, Wis. (NTA, 1790 Broadway, New York 19.)

26–31. International Cong. of Comparative Pathology, Lausanne, Switzerland. (Prof. Hauduroy, 19, rue Cesar Roux, Lausanne.)

30–3. International Hospital Federation, 9th cong., Lucerne, Switzerland. (J. E. Stone, 10 Old Jewry, London, E.C.2, England.)

31–1. Chemical Inst. of Canada, Quebec, (Donald W. Emmerson, CIC, 18 Rideau St., Ottawa 2.)

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Orchestral Acoustics

E. G. RICHARDSON

Dr. Richardson, a member of the physics department, University of Durham, King's College, Newcastle-upon-Tyne, England, is also an adviser and lecturer in architectural acoustics. He has published several books and articles on sound, orchestral instruments, fluid dynamics, and physical science in art and industry. During World War II he was a member of the scientific staff at the Admiralty and at the Royal Aircraft Establishment.

MUSICAL acoustics is one of those peculiar disciplines that border on both the arts and sciences. For a long time, the design of orchestral instruments was the domain of the musical craftsman who proceeded by cut-and-try methods, with only an intuitive knowledge of physics to help him and only his ear to guide him.

In the last 50 years electronics has revolutionized this process, just as it has so many other branches of applied physics; and even if it has produced no new music-maker more startling than the electrophonic organ, it has put at the service of the instrument maker electronic equipment with which to test precisely the effects of acoustical improvements in the sound sources that he fashions. Not, of course, that one should decry the ear as the ultimate arbiter—after all, a concert audience does not consist of a bunch of cathode-ray oscillographs—but the scientist feels he is getting somewhere when he finds a famous organ builder introducing oscillographs, wave analyzers, and sound-level meters into his factory.

It is, indeed, not surprising that the empiricists had accomplished much by the middle of the 19th century, but then look what a long time they had been working—since the times of the ancient Egyptians and Greeks certainly. On the other hand perhaps there is not such a large step between the trumpet in Tutankhamen's tomb and the trumpets used in Duke Ellington's band. Although we have contributed something, as I shall endeavor to show, we who work in electroacoustics have not been able, up to the present, to do much more than dot the *i*'s and cross the *t*'s for the musical craftsmen.

One of the difficulties of this study, as I have just hinted, is that it involves several different branches of science. It is not just applied physics, and he who tries to treat the ear as a galvanometer, always registering the same response to the same stimulus, is oversimplifying the tasks of musical acoustics.

Before we can start assessing the beneficial effects that alterations in the build of a musical instrument may produce, we must have means of precisely measuring pitch (or frequency), loudness (or intensity), and quality.

Someone may object that all this can be done by the trained ear unassisted by scientific paraphernalia. Although it is true that the ear can analyze quality in the way that we want, it is difficult, if not impossible, to use it as a precise measuring instrument. There are many people who cannot say when the pitch of one note is twice that of another, and even trained musicians find difficulty in saying when one note is precisely twice as loud as another of equal pitch; trained musicians have still more difficulty when the pitch is not the same. We experience the same difficulty in using the eye to test relative brightness or the sense of touch to measure pressure.

For these and other reasons, the scientist is chary of asking of his sense organs anything further than to tell him when two marks lie exactly over each other! This accounts for his fondness for having pointers that move over dials on his apparatus. Thus, in the apparatus described in the following paragraphs, we replace the ear by electric devices that have dials for measuring frequency (for pitch) and intensity (for loudness)—devices that can be operated by a person who is stone deaf. I am afraid I do not have the space to go into the question of the calibration of these instruments to insure that their response corresponds to that of the average ear, but it is an extremely important matter.

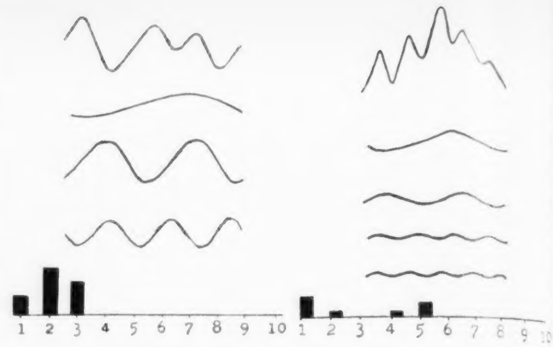

Fig. 1. Examples of acoustic analysis: top, complete wave forms; middle, several components of the wave forms; bottom, spectrums of the complete wave forms.

Analysis of Musical Sounds

In our sound laboratory at Newcastle-upon-Tyne, we use several methods of sound analysis, but in the main we prefer one of two according to whether the sound is to be analyzed while it is being produced or whether a record is to be made for subsequent analysis. In the latter case, we record on film, using a ribbon microphone, amplifier, and cathode-ray oscillograph, so that the record is obtained as the trace of a single black line on a white background (or vice versa) corresponding to those shown in Fig. 1.

In the oscillograph, a beam of electrons is usually shot onto a luminescent screen but is made to move up and down in step with the sound waves picked up by the microphone after the sound has been converted into electric waves and amplified. In the camera that photographs the movement of the spot formed where the electrons hit the screen there is a sensitive film moving horizontally at constant speed. The resultant wave form developed on the film is a combination of the vertical movement of the spot on the screen and the horizontal motion of the film.

Before the film can be analyzed, it must be converted into a record of the "hill-and-dale" type by blackening the whole of the film that lies to one side of the wavy line, leaving the other side transparent. This is done on the positive from the negative obtained in the camera. It may be noted, by the way, that we could get a record of the "hill-and-dale" type directly by using equipment of the "talking-film" type, but we prefer the oscillograph because of its more faithful reproduction, in spite of the labor involved in blackening a short length of film.

A short length of the film comprising three or four complete wavelengths is wrapped around a glass cylinder (Fig. 2, *C*), at the center of which is the filament of the special lamp *L*. By means of the constant-speed motor, *E*, the glass cylinder is rotated, and as this happens the lamp shining past the film and the slit *D* casts an image of a narrow section of the film on the photoelectric cell *P*. The wave form on the film is, thus, converted back into synchronous electric waves in the circuit containing the photoelectric cell and the analyzer *A*.

This electric wave analyzer is like a wireless receiving set in principle, but it covers the gamut of sonic waves instead of the ultraradio frequency waves that are used to "carry" broadcast music. One proceeds to tune the analyzer through its frequency range of 20 to 20,000 cy/sec, picking up the various harmonics of the wave form and noting the readings of the voltmeter that records the intensity of each constituent signal. (This instrument gives, in fact, an indication like that often given on a radio set to indicate optimum tuning to a station's frequency.)

If the sound to be analyzed is a musical sound of constant pitch and intensity, the current from the recording microphone is taken directly after amplification to the wave analyzer without the intervention of the film; but if we are dealing with transients, we have to make a film and analyze a short portion of it.

Figure 1 shows two examples of this analysis for two comparatively simple sounds. The top curve shows one complete period of the wave form as

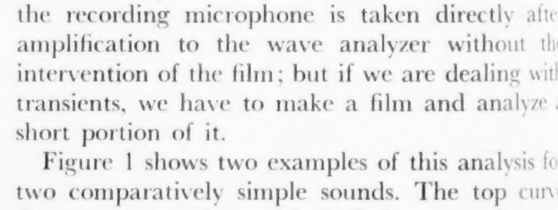

Fig. 2. Photoelectric analyzer.

recorded by the oscillograph. Underneath are three or four simple wave forms into which the original was analyzed by the machine. These are also shown in relative magnitude by the height of the black rectangles on the "acoustic spectrogram" at the bottom. In these two cases the components were true harmonics and were comparatively low in the series. When the intensity of a musical instrument is raised or when we are dealing with complex systems like a bell or the human voice, the components are no longer all harmonic to the fundamental and, moreover, they cover a greater portion of the musical scale so that our analysis may extend to the detection of a score or more of partial tones within the frequency range of the apparatus.

The Orchestra and the Auditorium

It is, of course, impossible to dissociate the acoustics of an orchestra, considered on a whole, from the building in which it plays.

Until the 16th century the Church remained the principal source of music performed in public. It was unfortunate that worship became associated with a type of building which, although it was capable of great beauty in the esthetic sense, brought with it not only a "dim religious light" but excessive reverberation, with the result that seeing and hearing became equally difficult for the congregation. The organ lent itself to the latter condition by having one of the slowest on-and-off speeches of all musical instruments, and the tradition became tolerated that every note and syllable in the church service must be long drawn out. An early print suggests that the need for amplifiers in the form of megaphones was even then found necessary to convey the speaker's voice to distant auditors.

The first concert music appeared in the houses of noblemen in Venice in the 16th century. The instruments were mostly viols, and if these were played in small music rooms to a select audience, conditions approached those that we may get in musical "at-homes" nowadays. If, however, performances were attempted in the lofty halls of contemporary Italian palaces, it should have become apparent that the precision of the string music was lost. Whether or not that was true, interest in acoustics was reawakened, and we find Athanasius Kircher publishing a treatise on it in 1675, although he was more concerned with the vocal than with the instrumental aspects of the question. It was not, however, until the beginning of the last century that, with the march of science, serious consideration was given to the design of acoustically good auditoriums. The name of Adolphe Sax is not revered by some serious musicians, but he was a man with ideas in advance of his time, and it is no fault of his that his design for a new wind instrument was put to—musically—base uses a century later, while his design for an acoustically good concert hall was never used at all (Fig. 3).

During the 19th century the tradition of the Leipzig Gewandhaus as the model for good acoustics took root. There was considerable justification for this, and the lessons learned from this famous concert hall have not been lost upon the architects of today. The feature that gave the Gewandhaus its good acoustics was the abundance of wood in the decoration of the interior. On the side walls, wooden panels lent resonance, and on the ceiling, broken up as it was by a ramification of massive king posts and queen posts, there was a reasonable diffusion of the sound and an absence of prominent echoes in spite of its height. For those who could not design acoustically well either by instinct or by copying a good model, there remained the pseudo-scientific palliative of the wire network. At one time, halls of excessive reverberation had miles of this perfectly useless wire stretched across from side to side at picture-rail level.

The American scientist Joseph Henry enunciated in 1856 the fundamental considerations that determine the acoustics of an auditorium. The relevant considerations are the size of the hall, the strength of the sound, the position of the reflecting surfaces, and the nature and material of the reflecting surfaces. The way in which each one of these four factors contributes to the auditory effect was investigated in a set of classical experiments by Wallace Sabine of Harvard, who made gradual changes in one factor at a time while keeping the other three constant. He used an organ pipe blown at constant pressure as the source of sound and measured the time that elapsed from the instant at

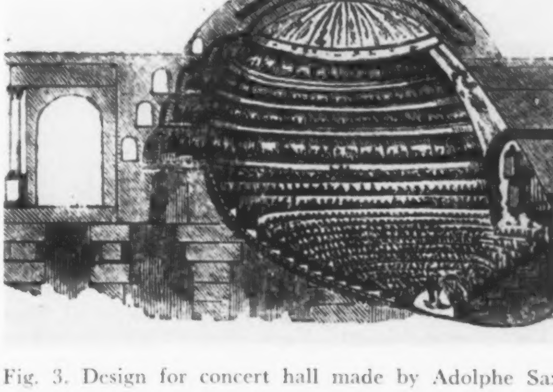

Fig. 3. Design for concert hall made by Adolphe Sax.

which the wind supply was turned off from the pipe until the sound was inaudible. This measurement, which he called the time of reverberation, he found to be proportional to the cubic capacity of the bare hall, while, when he introduced absorbing material in the form of cushions in large numbers, he found that the time of reverberation was in inverse proportion to the quantity of absorbent introduced.

Tonal Balance

Assuring the correct reverberation is not, of course, the whole desideratum behind concert-hall acoustics, but it is very important, and there are other details to consider before we pass on. The most important of these is the variation in the time of reverberation with pitch. Porous materials show smaller variations in this respect than do harder materials, which tend to be selective, especially if they are so attached to the walls that they can "drum" under the action of notes of the pitch to which they exhibit resonance. Porous materials on the other hand tend to absorb sounds coming onto them perpendicularly better than those incident obliquely, which is undesirable. Even so, the absorption shown by porous substances, such as the clothing of the audience, may vary by 3 times from extreme bass to treble. Usually the absorption rises to a maximum and then falls again going up the musical scale, so that, to get uniform absorption over the gamut, some substances having an absorption maximum in the bass and others with the peak in the treble must be introduced. The former may be wood or fiber panels with an air space; the latter, fibrous plasters applied to the walls.

It is clear that, on this basis, a different reverberation time will be desirable for different types of music: piano, organ, voice, quartet, or orchestra. To a certain extent such differences may be assured by hanging textiles that may be fully exposed or more or less rolled up.

Two additional factors that apparently have been greatly underestimated in the past are the ear's sensitivity to the direction and distance of the sound source. The incident sound waves produce sensations not only of the direction but also of the distance of the sound source. The sensation of direction is known to be the result of the time interval between the waves' arriving at the two ears; the sensation of the distance, on the other hand, is produced by the low-frequency transients. These low-frequency transients are produced by the building-up and the dying-out of musical sounds and they continuously interrupt the steady musical tones. They produce a sensation depending upon the curvature of the wave front—that is to say, upon the distance of the source.

It is not difficult to demonstrate the fact that the low-frequency transients are responsible for the distance sensation. We have to use only a ribbon microphone, which, like the human ear, exhibits a certain percentage of velocity reception, and to use a simple magnetic telephone for reproducing the sound. The magnetic telephone usually has a fundamental frequency above 300 cy/sec, and its response falls off at low frequencies. However, and this is the important point, it will not reproduce low-frequency transients; for this reason the low-frequency transients of the original sounds will not be distorted by the telephone. If we speak into the microphone, the listener in the other room will have the impression that the speaker is talking into the listener's ear. If we suppress the low frequencies by means of a high-pass filter—that is, cut off frequencies below 300 cy/sec—the speaker will seem to have moved suddenly to a great distance.

Of course, the impression of distance is only a relative sensation, not an absolute one. A hidden sound source particularly rich in low-frequency transients will always be judged to be in the vicinity. On the other hand, the sound that originates from a source deficient in low-frequency transients will always seem to be at a great distance. To demonstrate this we may compare the sensation produced by a sinusoidal vibration with that produced by a warbling tone. The sinusoidal oscillation sounds faint and thin, as if the source were a long way off; the warbling tone, however, seems to originate from the immediate vicinity of the ear. We may note a remarkable musical quality of the warbling tone in comparing it with the unreal sinusoidal vibration.

The transients not only are responsible for the sensation of distance but also create a particularly pronounced sensation of the direction of the source. This may be readily shown by simple experiments. We may hide a few loudspeakers and drive them by sinusoidal currents; a person placed in the middle of the room will hardly be able to locate a single speaker correctly. If, however, we switch the current on and off, there is no difficulty in guessing the exact location of the speakers. By switching the current on and off we excite the transients of the speakers, and these characteristic features make it possible to localize and to distinguish the speakers from one another. The transients are thus recognized to represent the direction and distance marks of our sound sources and therefore enable us to localize the various instruments in the orchestra and to distinguish them in the general sound pattern. They are the main reason why the sound of an orchestra does not blend into a single composite tone but maintains the individual features of the various musical instruments.

And furthermore, it can be shown that the tran-

sients raise the quality of the vibration of a musical instrument to that of a musical tone and that they are decisive with regard to the brilliancy of the playing. If we watch the picture of the sound of a musical instrument on the screen of an oscillograph, we see the peaks and all the other parts of the oscillogram continuously changing their amplitudes and positions, however steadily we may try to bow the violin or blow the trumpet; on the other hand the screen picture made by an electric generator would remain the same all the time. The musical sound is, thus, something alive, a sound continuously changing in its details. And this liveliness is the feature that distinguishes musical sounds from electric imitations.

To obtain the musical effect of a sound pattern, it is not so much the hearing of the distance and direction that matters, but the hearing of a certain acoustic "width" and a certain acoustic "depth." If we suppress the low frequencies and hence, also the low-frequency transients, the sound pattern will lose its acoustic depth and will thus produce a stale and thin impression, like the reproduction of a radio lacking in low-frequency amplification. The conditions are similar in the open air or in highly damped halls, where the loudness because of the lack of reverberation, greatly decreases with distance. Owing to the peculiar characteristics of the human ear, the subjective loudness of the low frequencies always decreases at a higher rate with distance than that of medium-pitched tones, with the result that the sound picture becomes poor in low-frequency transients when we go away from the source. This also seems to be the reason that, in the open air, we always have to use trumpets, drums, and other instruments that are rich in low-frequency transients.

In a large hall also, some of these low-frequency transients are lost, and we must try to make up for them by increasing the amplitudes of low-frequency steady tones. This is why the orchestra in a large concert hall needs plenty of bass and why the organ pipes in a church must be large if the nave is large. The musician implies this by saying that the orchestra or the organ has to be "tuned to the resonance of the hall."

Strings

The stretched string set in vibration is one of the oldest types of musical sound producer. The string, of gut or metal, being put under tension, may be set in vibration by bowing, plucking, or striking. Of these, the first is, from orchestral aspects, the most important, since it gives the foundation tone of the orchestra. At first known in the chamber orchestra of the 15th and 16th centuries under the name of viol, this instrument was given greater intensity of sound and color in the form of the violin, invented about 1550 in Italy. These improvements were obtained by increasing the tension and adding a sound box and board. Viol-type instruments were made in various sizes of which the violin, viola, and violoncello remain.

The shape of the violin is sketched in Fig. 4. Prominent features are the holes through which the air vibrations communicate with the atmosphere, the bass bar *BB* that is designed to give strength to the upper wood to which it is glued, and the sound post *S*, which lies under the bridge (usually under one foot) and prevents the case from collapsing under the pressure of the bridge, which in turn supports the pressure of the strings in tension.

Each of the stringed instruments has one main resonance resulting from the air cavity and another ascribed to the body. In the violin, these resonances are, respectively, near 300 and 500 cy/sec. Although these resonances are apparent in the frequency spectrums of both good and bad violins (Fig. 5), it is undesirable to have them unduly prominent. Especially on a poor violin, when a tone near that natural to the belly is elicited, coupling between the two results in an uncontrollable wobbly note, which has earned the name of "wolf." The belly then executes a sinusoidal motion of large amplitude, whereas at frequencies a little above or below, its motion is quite complex. A fact that does not seem to be explained is that the wolf note is usually observed at the upper harmonics of the wooden system of the violin and not at the fundamental.

The various resonances should, of course, be fairly evenly spread over the playing range—five octaves above the G string in the case of the violin.

Cheap violins lack intensity in the lower register, but this fault may often be corrected by the maker. The quality is affected by well-known adjustments: the position of the sound post and the height, weight, and form of the bridge are all generally subject to variation. The right foot of the bridge, which stands over the sound post, hardly moves, but the bridge tilts laterally about this foot, thereby transmitting string vibrations to the belly through the bass bar. (It may also tilt longitudinally if un-

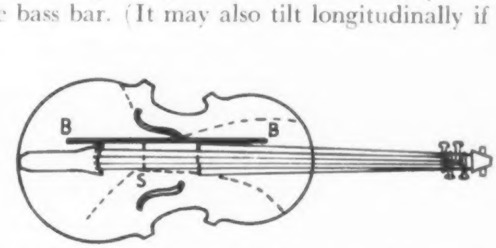

Fig. 4. Form of violin showing bass bar (*BB*) and sound post (*S*). The dashed lines indicate typical nodes of vibration.

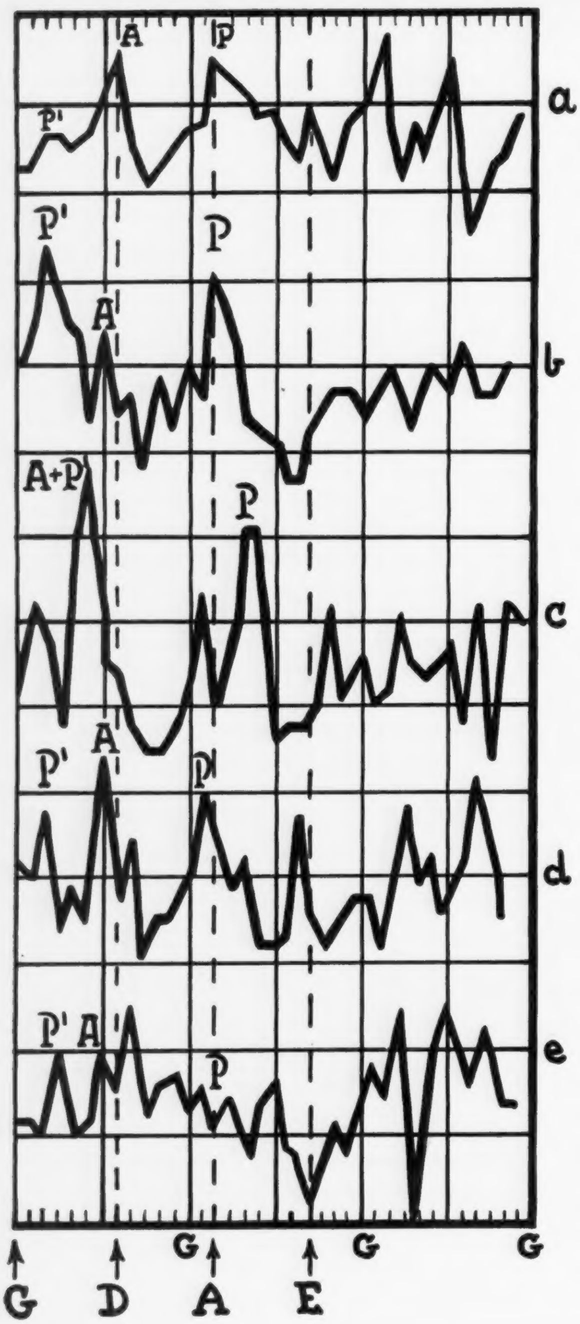

Fig. 5. Response curves of several violins [redrawn from F. A. Saunders, *Violins and Violinists*], loudness vertically, frequency horizontally. Arrows indicate open strings. *a*, an Andreas Guarnerius with good peak-spacing but some weak spots. *b*, an expensive old Italian violin, weak above A 440. *c*, an unfortunate type among cheap and expensive violins both. The coincidence of *A* and *P'* leaves a bad D string valley. *d*, same as *e* after treatment. Average level 2.5 units higher. Both *P* and *A* (the air peak) better. The overtone peak *P'* helps out the low G string tones. *e*, original curve of common, cheap violin. Note weakness of *P* and bad valley around E string.

pleasant shrill longitudinal vibrations of the bridge are elicited, but it is so positioned that it does not readily pass these on to the belly.) The vibrations of the strings have to pass through a narrow "waist" in the bridge to reach the feet, and filing this waist has a profound effect on the acoustic spectrum, since it takes away any shrillness of the upper partials.

Tone-color analyses show that the tones at the peaks are rich in the fundamental tone or the lower overtones. These responses have a great deal to do with the total sound given by a violin. Figure 5 shows a few response curves obtained by F. A. Saunders and Mrs. Hutchins. The peaks are the result of the natural vibrations of the top plate of the instrument and of the included air. The principal one (*P*) is a body vibration and coincides with the wolf note if present. There may be 6 to 10 lesser peaks scattered apparently at random between *P* and the highest notes of the violin. A plate of wood sounds when it is held at certain points and tapped at others. The top of a violin can give several tones when it is held at certain points; but after gluing it will have somewhat different natural tones because it is part of a larger structure and is no longer quite free. When the string sounds the pitch of any one of these natural vibrations, the violin responds loudly, even when the natural vibration coincides with one of the harmonics of the string tone.

The peak of lowest pitch (*A*) is caused by the vibration of the air inside the box, rushing in and out of the two f-holes together, at a frequency of about 280 cy/sec, or near the low C sharp. This peak is rather wide and it greatly improves the tone of several G-string notes by strengthening their fundamental components. When the air vibration is stopped by soft cotton plugs in the f-holes, this peak is much reduced. Its pitch can be raised by lessening the volume of the box or by increasing the total area of the f-holes. The body vibrations can have their pitch lowered by thinning the top of the violin or by loading the bridge with a mute. The spacing of the air and body peaks is very important. A proper spacing leads to a more even scale and to strong low tones.

It is also possible to find a bad arrangement of the peaks, even in a Stradivarius. Suppose, for instance, that the air peak *A* in a violin comes near the low C on the G string, and the peak *P* at an octave above. The overtone peak from *P* then coincides with the air peak (Fig. 5, *C*). Such a violin has many relatively poor tones; it needs to have its *P* lowered, perhaps by a thinner top.

Let us consider the viola for a moment. Saunders

thought, as Savart did in 1830, that the air peak of a viola ought to occupy a position similar to the one that is so successful in the violin. This would place it at F sharp on the C string, instead of at its usual position about a major third higher. When he got a viola into this condition, the result was disappointing. The C string boomed with a soft tone, quite unlike the rest of the instrument. This instrument was discounted by several competent musicians because it did not sound like a viola. To reach the low air peak that Savart desired, the f-holes had to be so small that the air met with a good deal of friction in vibrating in and out of the holes, and this weakened the air peak. To overcome this difficulty, one would have to increase the depth of the viola so much that it would no longer fit under the chin; this Savart idea was finally abandoned.

The latest improvement introduced by Saunders is to thin the top plate. The edge is thinned only in the parts of the top that are easily reached. Under the tailpiece and the fingerboard, the wood is left strong, in order to bear the tension of the strings. No thinning is needed near the f-holes because these free their part of the top themselves. He now recommends thinning a strip on the inside of a violin top that is 1/4 in. wide, reducing the thickness to 5/64 in. Since making the first thin edges, he has produced several new violas with this feature; and several other violas, violins, and 'cellos have been thus treated. In every case the instrument has been improved, in most cases very much.

The physical differences between good and mediocre violins seem, from the work of Saunders in the United States and Meinel in Germany, to be somewhat intangible. In fact, to many people, these differences do not exist; audiences of musical critics often are unable to pick out a Stradivarius from other good modern violins if they can only hear and not see the instruments. Saunders maintains, however, that an experienced player can tell when he is playing on a good violin, although he may not be able to express the difference in words. In part, it seems to be a matter of ease of tone production or "singability" in an instrument; but the transients, which have not yet been studied thoroughly, also seem to play a part.

I have already mentioned the important part that transients play in differentiating instruments of different families. Skudryzk opines that the old masters and their good modern equivalents radiate these transients (particularly in the low frequencies) better than less good violins, making the former stand out better in concertos. Kurz, on the other hand thinks that the *Klirrfaktor* (nonlinear amplification) is important for this differentiation and compares an old master and a mass-produced violin in this respect, finding that the former has a pronounced distortion peak about middle C.

Wood of great age shows a particularly small internal friction at low frequencies. The sound of old violins will, thus, be very rich in low-frequency transients; and their tones will, therefore, be very plastic and will always be distinguished from that of the other instruments in the orchestra. The old violins may sound feeble in the proximity of the player but, listened to from a distance, they seem to overpower the whole orchestra. The internal friction of the aged wood increases with the frequency, causing the higher overtones to fall off greatly in amplitude, which gives a sound particularly soft and agreeable. For new woods, the internal friction at low frequencies is larger, at high frequencies smaller. Modern instruments will thus tend to be smothered by the orchestra and will sound more harsh, but on account of their smaller friction at high frequencies they will produce a more lively, brilliant impression.

Varnishing has an effect similar to aging. Its object is to increase the friction at the high frequencies so that the instruments will show a soft sound quality. The low frequencies must not be affected by the varnish; otherwise the quality of the instrument will decrease. With increasing age of the instrument, the varnish becomes stiffer and its damping will gradually be reduced. By then, however, the quality of the wood will have increased by a corresponding amount, and the varnish will no longer be required. Another important factor is the mechanical distortion of the body of the violin. To minimize this distortion, the violin is fitted to the curvature of the outer portion of its resonating body and is reversed to the curvature of the inner portion; thus the system forms a mechanical "push-pull" arrangement. The main purpose of the curved parts, however, is to increase the radiation of the low-frequency transients and make the body act as a solid piston at low frequencies.

Skudryzk thinks that the reason for the qualities of old Italian violins is not lost secrets or recipes for varnishing but is to be seen in the highly skilled labor and the great talents of Stradivarius and his successors, which in modern life are not likely to recur.

The case of the struck string presents many points of similarity to the plucked string. We should expect that a hard, sharply pointed hammer would produce a kinked wave and a consequent note rich in upper partials, and that a soft, rounded hammer would produce a hog's-back type of wave and a

more mellow tone. In point of fact, these expectations are realized, for the main difference of quality between the note of the harp and that of the piano is the result of the suddenness of the displacement of the piano wire. Under the comparatively slow motion of the finger, a plucked string has time to adjust itself to the new forces before the finger lets go, and just before this happens the whole wire is momentarily at rest. But the blow of the hammer lasts only for a time comparable to the time of oscillation of the string. Photographs show that a piano hammer remains in contact with the wire for less than the time of one vibration before it flies back. Under these circumstances the struck point moves in advance of more remote parts of the string, which may have only started to move before the blow is finished and the struck point is beginning to return. This naturally complicates matters to an extent depending upon the force of the blow of the hammer. The force of the blow can be increased by increasing the mass of the hammer and by allowing it to hit the wire at greater speed; these expedients are found to increase the intensity of the fundamental tone of the wire if the blow is applied, as it is in the piano, near one end of the string.

As a matter of fact, the striking point chosen is one-seventh of the length from one end. It was formerly thought that this had the effect of removing the objectionable seventh harmonic. In fact, the striking point seems to be so chosen that the amplitude of the fundamental is large and the forced vibrations of the soundboard across which the wires are stretched is at a maximum, whereas the natural vibrations of the soundboard—those which are excited by tapping the board and which the maker desires to eliminate—are reduced to a minimum.

Four decades ago the Physical Society of London held a discussion on that elusive factor in pianoforte technique called "touch." The opinion prevailing then among the disputants was that it was a fetish set up by the virtuosos for their public to worship. This view has received conclusive confirmation from the work of Hart, Fuller, and Lusby at the University of Pennsylvania. They made oscillograph records of the notes produced by two American virtuosos on a piano and a set of records of the notes produced by a pendulum which on release hit the same piano key. In every case, it was possible to get an exact match of one of the human records with one of the robot's. The inference is, in the words of a speaker at the Physical Society discussion: "The only factor that a player can vary is the speed with which the hammer hits the string."

Percussion

The soundboard of the piano fills an important function, even more so than that of the violin. As far as possible, it must vibrate all in phase and not with some parts moving in while others move out, since the vibrations would then tend to cancel one another's effects at a distance. This will not be possible at high frequencies but it is helped by having a board with a high elasticity (hence, velocity of sound) and low internal friction. Young has shown that after the initial peak on striking there occurs a fairly uniform decrease in intensity at a rate greater for the higher partial tones, so that the sound becomes simpler as it decays.

The same thing is shown by the church bell. The clapper strikes at the sound bow, the section which, owing to the change from divergent curves to convergent, has the greatest thickness. Figure 6 is a photograph, credited to the late Taber Jones, giving the wave form of such a bell at various epochs after striking.

The "strike note" of a church bell is the tone that is most prominent when several bells sound in succession, and this is the tone that is meant when the pitch of a bell is given. To the ear, this prominent tone is about one octave below the fifth partial. In spite of study by various experimenters, the question of how the strike note is produced is not yet settled.

On several bells, Rayleigh found that the pitch

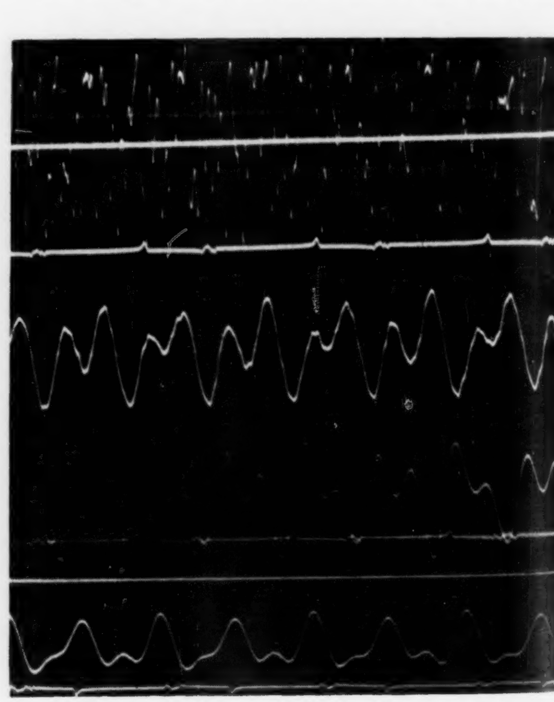

Fig. 6. Wave forms of bell, 0.1, 3, 7, and 12 sec after striking. [Taber Jones]

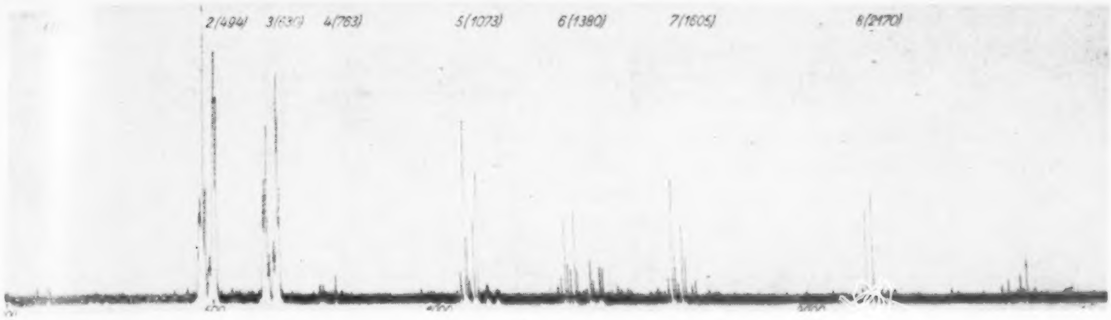

Fig. 7. Spectrum of bell just after striking. [Meyer and Klaus]

ascribed to the bell was not given by any normal mode of vibration of the bell and that this could not be reinforced by a resonator. In each case, he found that the strike note was about an octave below the fifth partial tone of the bell, and on a bell that he was able to examine with some care he found that this fifth partial is heard when the bell is struck on the sound bow but soon falls off when the locality of the blow is varied and, in the upper three-fourths of the bell, is very faint.

Meyer and Klaus tested whether the strike note is of subjective origin—that is, whether the pitch of the strike note is determined by the fifth partial but for some reason is judged to be in the lower octave. For this purpose, the sound from the bell was picked up, amplified, and sent to a loudspeaker by means of a system that was very free from distortion. Electric filters were employed to cut out chosen components of the sound. When only the first five partial tones from the bell were transmitted, the strike note was not heard, but when the seventh was also transmitted the strike note was heard clearly. This experiment seems to show that the seventh partial, in addition to the fifth, plays an important part in the production of the strike note.

At one time Meyer and Klaus picked up the sound from the bell with a condenser microphone. The response of the condenser microphone was linear, and the analysis showed no trace of a component at the pitch of the strike note. At another time they made use of a carbon microphone with a response curve somewhat similar to that of the human ear, and in this case a tone of considerable intensity was found at a frequency of about 533 cy/sec—that is, about the pitch of the strike note.

Moreover, it is well known that a simple tone may often be judged an octave higher or lower than the pitch given by its actual frequency. Thus, it is possible for a difference tone from the fifth and seventh partials of a bell, introduced by the nonlinear response of the human ear and lying in the neighborhood of an octave below the fifth partial, to be judged as that of a tone an octave lower. This at least seems at present to be the best hypothesis about the origin of the strike note.

Figure 7 shows the spectrum of a bell taken just after striking. The lowest possible frequency or hum note (1) is usually weak, the fundamental (2) being loudest. Above these come various weaker overtones, but the strike note, which should occur at 530 cy/sec on this bell, is absent.

The drum is a simpler type of sound source than the bell and has harmonic overtones. Often it consists of two membranes stretched over the ends of a cylindrical barrel, the air column in which couples the two membranes in vibration. It is the practice to hit the drum skin at one-quarter to one-third across the diameter, since this encourages the fundamental of the skin, but the pitch of the latter is affected by the cavity resonator to which it is coupled.

Wood Wind

The development of wind instruments from their very beginnings may be followed even now in the sound-producing apparatus of primitive peoples. However savage a race may be, some form of drum and some form of whistle will be found among its tribal equipment. The genesis of a whistle is a reed cut from the living plant with the pith removed or a straw. If one end is stopped, a sound can be produced in the same way that one can excite the vibrations of the air in a door key.

Since reeds are rather frail, especially if long, the next stage was to make an artificial pipe by hollowing out a piece of wood or even molding a pipe of baked clay.

This pipe was scarcely a musical instrument, but the ingenuity of man soon allowed him to play a succession of different notes by tying together a number of such pipes of graded length, so that the breath could play upon each in turn or in any desired order, the whole instrument being moved across the mouth as in playing the modern mouth-organ. This instrument, the Panpipes, or Syrinx

(Fig. 8), is of great importance as the earliest type of instrument able to produce a musical scale. Early organs, were, in fact, little more than a portable form of Panpipes blown by bellows.

Forming the lips to direct the air across the open end of the tube entails a certain amount of practice or fatigue, however, and attempts were made to help the player in this respect. Certain flutes have come down to us in which the open end had a nick cut out of it on one side to facilitate the forming of the correct embouchure on the part of the player. The latter's task was considerably lightened when the whistle form of mouthpiece was introduced.

We can see the development of such a mouthpiece in the flute made by the native races of North America, where the whistle is external to the pipe itself. A notch is cut out of the pipe near one end and a little wooden box is placed over it in such a way that air emerging from an aperture between the box and the pipe is driven across the notch toward its opposite edge. Another hole is made in the pipe to let the air into the box, while a plug prevents the player's breath, which now comes from between his lips gripping the end of the pipe, from passing directly up the pipe itself.

Following closely on the heels of the Panpipes was the invention of the reed pipe. Take a little piece of straw and partially slit it in a longitudinal direction; place it in the mouth and grip the fixed end with the lips and you have a new sound-producing instrument—the vibrating reed or pair of reeds, if both sections of straw are free to vibrate. The sound so produced is of feeble intensity, but man found that he could fit this into a "resonator"—a wider straw or pipe—and produce loud notes from the system formed by the vibrating reed coupled to the vibrating column of air in the pipe.

Another ancient form of wood-wind instrument that has survived in Celtic races to modern times is the bagpipe. The principal component of the highland bagpipe is a chanter, from which nine notes can be produced. The flow of air to the chanter is maintained and regulated by a blowpipe fitted with a valve and by a bag that acts as an air reservoir. From the bag project three drones, two tenor and one bass, which supply an unvarying harmony.

Near the air inlet to the chanter is a reed, a double vibrator made of Spanish cane bound to a copper staple. The characteristics of the note it produces vary greatly with the temperature and humidity of the air supply. The reed used in the drones is a piece of cane in which a single beating tongue has been cut; this is one of the oldest methods of maintaining a musical note.

The chanter is made by boring two conical holes

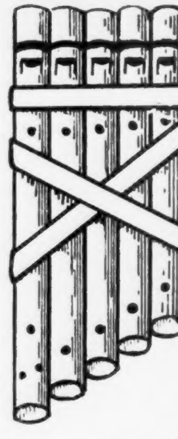

Fig. 8. (Above). Panpipes.

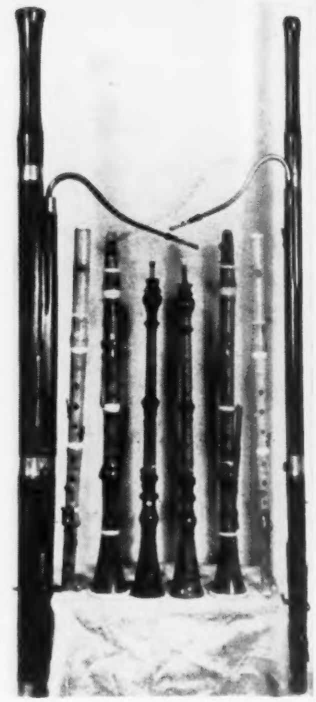

Fig. 9. (Right). Types of orchestral wood winds, about 1800. From outside to inside, pairs of bassoons, flutes, clarinets and oboes.

along the axis of a wooden rod, about 37 cm in length. The apex of the longer cone points upward (near the air inlet), and the apex of the shorter cone points downward. The cones overlap to give the region of narrowest bore, about 7 mm in diameter. The chanter has a series of seven holes along its length as well as the thumb hole, which is nearest to the reed and on the opposite side of the pipe from the other seven holes. Two cross holes near the lower end of the chanter are bored at right angles to the others. The drones are cylindrical tubes, with some parts widened to permit tuning.

The next stage in the history of these wind instruments was the boring of holes in the side of the pipe. Owing to the need of support for the instrument, it was not found feasible to employ more than six holes, three to be covered by the first three fingers of each hand, with possibly a thumb hole at the back. If these holes are all covered, we have the note of the complete column of air of the pipe. Uncovering the holes successively, starting with the one farthest from the mouth, we get a series of notes of rising pitch.

Much ink has been spilled on the significance of the positions of these holes for the formation of a musical scale by the ancients and by present-day primitive peoples, but at the risk of differing with the opinions of some eminent ethnologists, I shall venture to suggest that the positions for these holes were quite fortuitous, or, at the most, dictated by

considerations of symmetry and convenience, not by any preconceived notion of musical scale. The Red Indians form the lateral holes by burning them out with a hot stick, the positions being determined by measurements with the breadth of one or more fingers. If the savage finds it more convenient to place the holes for the right hand farther down in order to avoid interlocking the movements of the hands, he does so, and then the holes are grouped in two sets of three. I am aware, of course, that the ancient Greeks had theories of the musical scale, but I doubt that these theories were ever adequately put into practice. The difficulty of making a wind instrument in true intonation to any musical scale today, even with the modern perfection of apparatus, is great enough and it must have been impossible—except by sheer luck—to approach such a standard with the means available at that time.

In the great period of the development of the orchestra, the period from Bach to Beethoven, these primitive instruments began to acquire the form in which they are familiar in the orchestra of today, with keys operated by the fingers to enable inconveniently sited holes to be opened and covered. Figure 9 shows a group of wind instruments typical of the classical period (about 1800) in which—reading from outside to inside—there are pairs of bassoons, flutes, clarinets, and oboes.

No subject connected with the science of musical instruments has provoked greater controversy than the functioning of the flute and diapason organ pipe. The mode of coupling the edge tone formed at the mouth to the column of air has been of interest in aerodynamics and acoustics alike during the past 50 years. Recently there has been a revival of interest in edge tones as being a possible cause of the horrific noise put out by jet engines, but the mechanism is still the subject of rival theories.

When air debouches from a linear slit and falls upon the sharp edge of a metal or wooden wedge facing it, a vortex system is set up. In a way that is still rather imperfectly understood, a steady formation of vortices takes place at the slit so that the wavelength of the system—the distance between successive vortices on the same side—is equal to, or is a small submultiple of, the separation of slit and edge.

Taking the simplest case, a vortex leaves the outer wall of the orifice as the preceding one on the same side arrives at the edge (Fig. 10*a*). There is a minimum slit-edge distance l_0 for any given velocity of efflux V at which a tone can be produced. The frequency of the edge tone is given by the frequency with which the eddies strike the wedge. If they move toward it with velocity U, and l is the distance between the vortices in the same row, then $f = V/l$, or since $U = aV$ where a is constant, V/fl is constant.

When V is kept constant—that is, when constant pressure is maintained behind the slit—and l is increased beyond the minimum l_0, the pitch of this edge tone falls in accordance with this relationship until at a value approximately double l_0, the system becomes unstable and the tone, which is now the suboctave of the original, may suddenly rise an octave to what it was at l_0.

Experiments in which smoke is mixed with the emerging air show that the original spacing of the vortices is restored after the transition, but with twice as many vortices between slit and edge. Figure 10*b* shows the relative positions of the vortices just before the transition. After the jump (Fig. 10*c*), l resumes its original value, so that the width is halved and the original narrow vortex street is recovered. The edge must bisect the two rows of vortices if the tone is to be elicited; consequently, in the shaded area of Fig 10*c*, representing the space between the wide and the narrow street at the transition, only the graver tone can be elicited. In fact, after the jump has taken place, the pretransition tone can be brought back either (i) by moving the edge into the shaded region, or (ii) by pushing an obstacle from the side partly into the path of the blast and so deflecting the shaded portion of the stream onto the edge, as is done by means of a wooden "roller" in certain organ pipes.

A number of explanations have been offered for the regularity of this vortex system, but the one that seems most plausible—at any rate, for the low wind speeds used in wind instruments—is the one I originally presented after I had noticed a system of "secondary vortices" alongside the wedge starting at the vertex of the wedge and proceeding down its boundary layer. The reaction of these secondary vortices on the phase of the air pendulation as it leaves the jet accounts for the spacing of edge-tone vortices.

Figure 11 shows a section of the mouth of an organ pipe of the type known as diapason, made of wood. Wind under pressure is led into the foot and is deflected by the languet out of a slit at the lower lip of the pipe, in a broad jet that is directed upon

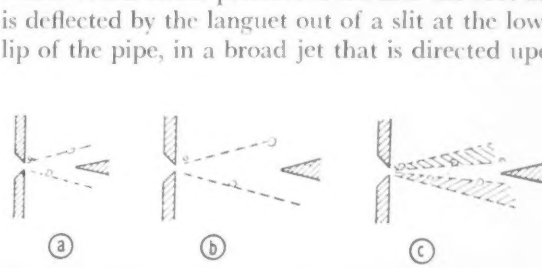

Fig. 10. Stages in vortex system accompanying edge tones.

the upper lip, formed by beveling down this side of the pipe. (This particular pipe has a double languet to "induce" air from the column into the jet.) Here then we have at the mouth of the pipe a system that is capable of producing this important avenue of eddies, which originate an edge tone at a pitch equal to the number of eddies that strike the edge of the upper lip in 1 sec. The distance between the slit and the edge that faces it plays a conspicuous part in fixing the pitch of the edge tone and is called the height of the mouth. A similar system is formed in the orchestral flute, with the "mouth hole," across which the player blows, as the seat of the edge tone.

The time is now ripe to consider these edge tones in relation to the rest of the pipe—that is, to the vibrations of the column of air. The tones that the column of air can give are quite definite. If the pipe is open at both ends, as in the orchestral flute, the column can give any or all of the members of the harmonic series. Let us now consider the effect of admitting wind into the pipe at gradually increasing pressure. As we have seen, an edge tone of gradually rising pitch would be produced in the absence of the pipe. But the column of air is trying to impose one or other of its natural frequencies upon the edge tone, and when the latter is near to the fundamental tone of the pipe, the two vibrate in sympathy. The column of air, being the stronger member of the partnership, is able to pull the edge tone out of its proper pitch to a certain extent; but

Fig. 11. Mouthpiece of organ pipe of flute type with double languet.

when the edge tone has slightly overpassed the pitch of the pipe, the latter will be pulled a little from its natural bent. At a greater extent of mistuning, the system will relapse into silence, to resume at a higher pitch when the edge tone approaches the next higher tone that the pipe can emit, namely, the octave. This is the mechanism of "overblowing" the pipe. Further increase of wind pressure will eventually cause this note to cease and the next overtone of the pipe to make its appearance.

It is evident then, that, depending on the width of the mouth, there will be a certain blast velocity and, therefore, wind pressure at which the flute pipe will function most efficiently. This will be the velocity at which the edge tone is exactly in tune with the pipe tone. Further, in a complete "stop" of organ pipes worked off the same pressure and so having nearly equal jet velocities, this theory shows that the width of the mouth should be made inversely as the pitch of the pipe, in accordance with the formula given in a preceding paragraph. That this is done is evident from an examination of any organ, since the width of the mouth is made a constant ratio to the effective length of the pipe. In a pipe of this type, it is usual to cut nicks in the lower lip and the languet in order to encourage the production of the eddies so necessary to the correct functioning of the pipe.

Of course, the timbre of a flute throughout its range is not constant—as we say, it has a "formant" —and even a stop of organ pipes all built to the same pattern shows variations in quality. Figure 12 shows some loudness-frequency curves, as a three-dimensional pattern covering the diapason ranks of five German organs, by which the variation can be seen going up the scale (front to back of the model).

The oboe, as well as a number of acoustically similar instruments, has a conical tube and at the mouth a double reed that the player grips with his lips. Its "tonality" is, therefore, like that of the flute; it has six side holes (with keyed extras) and overblows to the octave.

The clarinet has been more exploited by scientists than all other orchestral wind instruments, for it presents several features of interest. Figure 13 shows how the reed is gripped by the "ligature" (a metal band) to the "table," the beveled end of the ebonite or wooden tube. The player grips this mouthpiece between his lips folded back over his teeth. The tube is cylindrical, about 2 ft long and ½ in. in diameter. Like the flute, the modern clarinet is sometimes made of metal.

As the reed begins to close the aperture, a condensation starts down the tube but is reflected as a

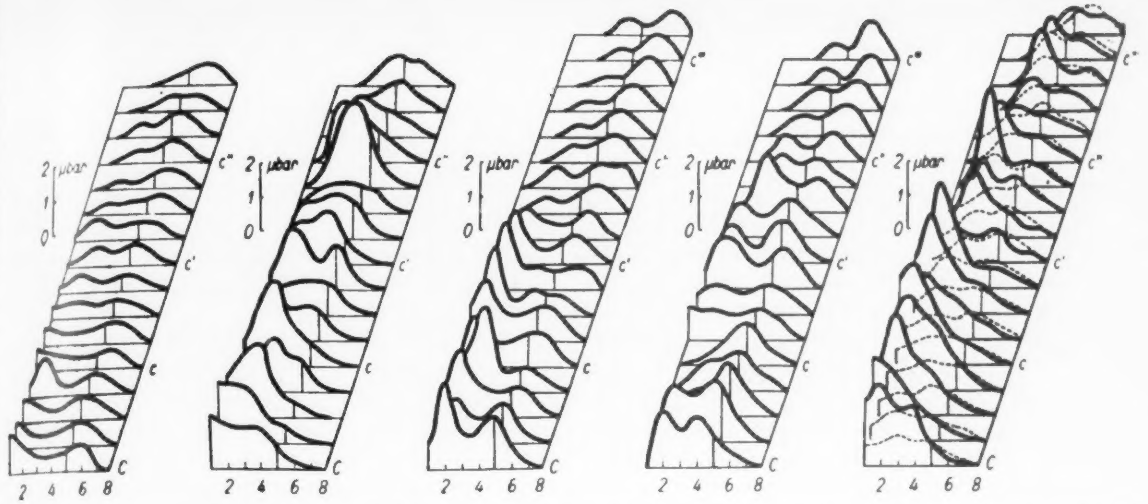

Fig. 12. Spectrograms of organ tone for pipes covering the whole gamut of frequencies: intensity is plotted vertically, frequency horizontally; pitch of pipes rising from front to back. [Grutzmacher]

rarefaction from an open side hole or from the distant end of the tube and, on return, finds the aperture closed by the reed. It is reflected, therefore, as a rarefaction for a second trip down the tube. Again reflected at the open end, this time as a condensation, it returns to push open the reed aperture. Thus, in one complete cycle of the reed's vibrations, the sound travels 4 times the length of the tube, as in the stopped diapason organ pipe. Accordingly, even harmonics should be missing in the timbre. The quality naturally changes with blowing pressure and with the effective length of the reed. The player can adjust this by the grip of his mouth, according to the pitch of the note he wishes to elicit, and to a certain extent pull the column out of its natural frequency. Natural vibrations of the reed, though feeble, can be detected in an analysis of clarinet timbre.

Since the column on the foregoing analysis behaves like a stopped pipe, it overblows to the third harmonic. To complete the musical scale, some additional side holes (compared with the flute and oboe) are necessary, and this entails somewhat more complicated fingering. This "bridge portion" between the lower and upper (overblown) register is the weakest feature of the instrument, both in intensity and in quality.

Brass Wind Instruments

With the exception of some obsolete keyed bugles and the ophicleide, brass wind instruments differ from the wood winds in that they have no side holes. The possible notes were formerly limited to the harmonic series based on the fundamental of the tube (the fundamental itself not being obtainable with any worth-while intensity on any but the smallest instruments). On modern instruments there are available, by the operation of valves, three or four extra lengths of tube to enable the player to cover the diatonic scale.

Exceptionally, the trombone with its telescopic slide allows continuous variation of tube length in place of step-by-step, but even so the player eventually confines himself to a number of set positions of the slide.

These instruments are also distinguished by the fact that the player's lips, held in a cup-form mouthpiece, take the place of reeds. For this reason, some acousticians refer to them as cup-mouthpiece instruments. Among themselves, they are distinguished by (i) the shape and size of mouthpiece, and (ii) the vertical angle of the quasiconical tube.

The reader will appreciate that the player's object is to exploit the natural resonances of the tube, and that this becomes more difficult as the flare of the tube increases. A wide-angled horn has, in fact, no resonances. This probably explains the difficulty of learning to play the French horn, especially in the upper register where the harmonics almost overlap, making it difficult to avoid "warbling" from one to the next.

On the other hand, the trumpet and, still more, the cornet, from the small vertical angle of the bore lend themselves to ease in exciting the natural tones of the column. In all cases the resonances will be relatively less sharp at the higher frequencies, be-

Fig. 13. Clarinet mouthpiece.

cause open ends will radiate more readily at high pitch.

It remains to discuss the acoustics of the type of pipe maintenance, peculiar to the brass. To a certain extent the scheme is an imitation of the oboe mouthpiece, in that the player's lips do the duty of a double reed. The lips are pressed by the ring-shaped rim of the mouthpiece, and by means of the osculatory muscle the player can alter the tension and vibrating length of these soft reeds. In the mouthpiece provided on modern horns this is the end of the story. The mouthpiece tapers gradually into the apex of the horn in a smooth funnel shape. On mouthpieces of the cupped class, however, the "reeds" are assisted by an edge-tone system, in that the issuing breath can be directed against a facing edge a short distance away.

The difference between the two mouthpieces is shown in Fig. 14, where the section of a French horn mouthpiece is shown (left) and one from a tuba (right), drawn to the same scale. The tuba is the largest of this class, the trumpet itself having a smaller mouth of the same shape. The mark of this category is a hemispherical cavity or cup, to the back of which the tube itself is quite abruptly inserted, forming a circular edge against which the ring-shaped eddies or vortex rings in the blast can be directed. A brass instrument having this kind of mouthpiece can, in fact, be made to sound if the circular or oval end of a narrow brass tube is placed at the position occupied by the opening between the player's lips when the lips are applied to the mouthpiece, and if a blast of air at suitable pressure is driven through the tube. Such an experiment shows that the vibration of the lip reeds is not essential, although it is true that the sound obtained is weak when the edge tone alone is used as exciting medium.

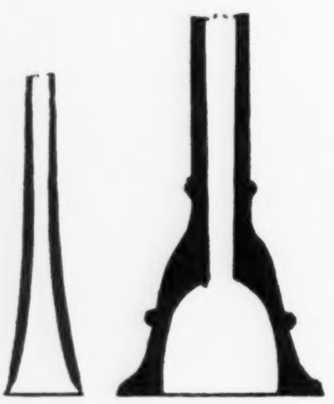

Fig. 14. Mouthpieces of French horn and tuba.

The resonant characteristics of the edge tones on instruments supplied with cup mouthpieces are important factors in determining the power and peculiar blare of the notes given out. When a mouthpiece of the conical funnel shape is supplied, the tone is soft, mellow, and less incisive, for it lacks a true edge tone, the missing resonance of which deprives the system of its power. On the other hand, this funnel shape leaves to the player greater facility in modifying the pitch, for he does not have to adjust three systems to a pitch that is more or less determined by the instrument. In the larger cupped mouthpieces, the edge tone approaches more closely that on the flute, for the shank of the tube is too wide to permit full use of its circumference. In such large mouthpieces, the player directs his breath to one side, using an arc of the circular edge, instead of its full circumference.

I hope it is apparent from this article that there still remains a great deal to study in this fascinating subject, particularly in the study of transient sounds, which could not be discussed more fully here. It is to be hoped that scientifically minded musicians, suitably equipped with apparatus, which, compared with that required in other branches of applied physics, is neither elaborate nor expensive, may be persuaded to fill the gaps in our knowledge.

I wish to thank a number of editors and scientists —in particular, F. A. Saunders—who have allowed me to use material from their publications in compiling this article.

Behind all your practical applications, there is a region of intellectual action to which practical men have rarely contributed, but from which they draw all their supplies. Cut them off from this region, and they become eventually helpless.—JOHN TYNDALL *in* Lectures on Light.

Statistics, Experiment, and the Future of Biology

R. E. BLACKWELDER AND L. E. HOYME

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When you can measure what you are speaking about, and express it in numbers, you know something about it; but when you cannot measure it, when you cannot express it in numbers, your knowledge is of a meagre and unsatisfactory kind; it may be the beginning of knowledge, but you have scarcely, in your thoughts, advanced to the stage of science.—Lord Kelvin.

STATISTICAL methods have proved fruitful in practically every field from advertising to zoology. Yet despite the fact that biometricians have contributed significantly to the development of statistical theory, biology as a whole is far from being a quantitative discipline. This apparent neglect of a useful tool has led to considerable criticism of the science. For example, in the July 1953 issue of *Endeavour*, Kenneth Mather quotes Lord Kelvin's comment, and adds,

> While it would be foolish to deny the name of science to any branch of study merely because it makes but little use of measurement, no science can reach its flowering until it becomes quantitative in both its observation and its theory. Simple description and qualitative comparison can lay a ground work . . . but sooner or later the change from qualitative to quantitative becomes essential if progress is not to falter or even to halt.

Others, such as J. B. Conant, go even further in suggesting that only an experimental field can properly be called a science.

The premise that the techniques used determine the nature and future of a science seems somewhat illogical. The more realistic approach to science selects techniques for their usefulness in solving the particular problems under investigation. Experiment and measurement have proved useful in many branches of science and may well be used more widely in biology than they are at present. But to imply that they are essential to the progress of biology, and indeed that they are essential to maintaining biology's claim to being a science, is similar to saying that everyone claiming a connection with law enforcement must carry and use a gun as proof. Biologists deal with a wide variety of problems, from the function of the stomata on a leaf to the evolution of great phyla.

Before insisting that biologists start setting up experiments and/or measuring things, it would be well to look at the requirements of the problems that biologists are called upon to solve. Are these procedures necessary—or even appropriate—for the solution of the problems? Indeed, are these procedures feasible? Each problem must be considered individually, but it is possible to give some general answers in the light of the nature of biology as a science.

Biology is in many ways a hybrid science, having much in common with both the exact and the social sciences. This duality holds both in problems and methods of approach. The social sciences—history, psychology, and sociology—are concerned with the recording of particulars in the light of factors influencing their development. The exact sciences—physics and chemistry—seek to progress from the particular to the general, and to derive universal laws. The techniques of the social sciences are primarily descriptive, for the particular situation to be explained has been supplied by a unique combination of chance or circumstance; and the conclusion drawn from the study is descriptive. The exact sciences, on the other hand, have in most cases passed from the descriptive to the experimental stage. Working with known raw materials under controlled conditions, the exact sciences are in a position to set up carefully planned analytic experiments and to draw conclusions that can be translated into generalizations. Both types of problem—the description of the particular and the

derivation of the general—are valid, and both types are found in biological studies. Both types, it may be added, are found also in the exact sciences, particularly in chemistry, where the description and qualitative analysis of substances, although expressed in quantitative terms, is an important area of investigation.

The diversity in the type of problems that biologists may investigate is a result of the inconceivable complexity of life. The duality of biology is evident in the history of the science. Like all sciences—indeed, all branches of knowledge—biology began with an attempt to understand things by describing them and by finding a classification for knowledge that would bring order out of chaos. As Darlington (*1*) describes the situation prior to the 19th century,

> . . . [the unity of theory] achieved by Newton for physics and by Lavoisier for chemistry, was long wanting in biology. Among living things, disunity triumphed everywhere. The only universal generalization in biology in its first two hundred years was that all living things were different. . . .

Then, toward the end of the 19th century, a number of phenomena common to all branches of the plant and animal world were discovered—among them Darwin's theory of evolution by variation and selection, Mendel's laws of inheritance, cell division, and the fertilization of the egg. However, instead of unifying biology, these new discoveries actually divided it further by creating new subsciences that differed from the older areas of biology in techniques and viewpoint. The traditional disciplines—taxonomy, anatomy, and embryology—are primarily descriptive and comparative, and they are concerned with particulars—the plants or animals produced by a unique set of historical, developmental, and other circumstances. On the other hand, the newer biological fields—genetics, biochemistry, and physiology—resemble the exact sciences in their attempt to pass from the particular to the general and in their use of experiments.

In the newer biological fields there is no doubt that quantitative methods were essential for the success of experiments and that experimental procedures, assisted by statistical analysis, led to significant progress. In genetics the interaction of experimental and statistical procedures has been particularly productive. Mendel's experiments, together with the utilization of the rudimentary statistical procedures of sorting the classes he obtained, counting the number in each class, and calculating ratios, led him to formulate the fundamental laws of heredity. Subsequently, the derivation of mathematical procedures for calculating expected results in experiments and for comparing them with the results actually obtained were essential for testing hypotheses concerning the inheritance of traits. And recently procedures have been designed for estimating gene frequencies and for comparing the frequency of a given allele in one population with its frequency in another.

Genetics presents an exceptional experimental situation because the data frequently involve only counts of the presence or absence of discrete traits. Experiments in other branches of biology are far less simple, for the traits under observation are usually continuous variables, such as size, and the organisms under observation are highly complex and variable. Since it would be both impossible and undesirable to standardize them for experimental purposes (the procedure of the exact sciences), Mather recommends that the variation be described by measurements and be stated in terms of average and standard deviation. A comparison of the frequency distribution of the control and experimental groups by statistical methods can then be used in evaluating the results of the experiment. The conclusions, admittedly, will be in terms of probability, that is, the probability that a distribution comparable to that of the control could be found in the sample by chance. Nevertheless, in this indirect way, the results of the experiment can be assessed. Some such method may well be useful in designing and evaluating experiments involving complex biological variables.

Tracing the evolution of phyla and genera and the reconstruction of former plant and animal worlds are biological problems of prime importance. Even such prosaic tasks as the identification, description, and orderly classification of organisms cannot long be ignored without grave consequences to other biological investigations. The fact that such problems are only indirectly susceptible to man-made experiment does not rule them out as legitimate scientific investigations. If one insists on experiment, one must recognize that nature is continually conducting experiments of its own to which man may turn for information on the changes in organisms through the influence of environment, the operation of natural selection, and, indeed, chance, particularly where time is involved. When man designs an experiment, conditions are carefully controlled and the number of variables is kept to a minimum. Nature's experiments, on the other hand, present an almost unlimited amount of data on a variety of subjects from which the observer must select the data pertinent to his problem. Aside from the agency providing the experiment, there is little real difference between a geneticist putting

fruit flies into bottles to study genetic drift with isolation and Darwin observing speciation in birds on isolated Pacific islands. In both cases a factor resulting in change has been introduced. In one case the extraneous factors are removed before the experiment; in the other case they are removed as the data are gathered. In either situation controls are available—in the former, by planning, and in the latter, by selecting a second test situation that parallels the first as closely as possible except for the absence of the crucial factor. The subsequent steps are the same: a question is asked, pertinent data are collected, and a hypothesis is formulated. The procedure for testing the inferences drawn from the hypothesis depends on the material at hand. In taxonomic studies, for example, if one is working with living forms, it is sometimes possible to determine by a few appropriate experiments whether two closely related populations are true species or merely ecotypes of the same species; with fossils or museum specimens, experiments are not feasible. When the conclusions have been based on large numbers of observations, checking may be less necessary; but when only a few data are available, further checking may be impossible.

Both experimental and descriptive biology are of necessity qualitative. Indeed, the majority of biological problems involve the form and size of organisms, whether one is describing a species, looking for alterations in form as a result of an experimental situation, or observing embryologic development. In recording one's observations, regardless of how they are obtained, measurement and appropriate statistical procedures can be useful as aids to description.

That it is desirable to express sizes and other characteristics in concrete, that is, numerical, terms, rather than by ill-defined adjectives, is obvious, and such data are customarily included in taxonomic papers. The characteristics that can be described numerically fall into three major categories: (i) measurements of continuous variables, (ii) counts of discrete phenomena, and (iii) the frequencies with which events occur. After numerical values have been assigned to the variates—length, breadth, weight, number of eggs, pulse rate, and so forth—these numbers may be manipulated by various formulas to arrive at new numbers describing the variability of the trait in the group under consideration. The formulas have been derived by experts and can be used by anyone with a basic knowledge of algebra. Apparently, then, the whole matter is very simple—just measure and calculate.

Measurement, however, is no simple matter. Lord Kelvin's statement that "when you can measure what you are speaking about . . . you know something about it" puts the cart before the horse. Unless one knows what one is measuring and why, one can waste considerable time. It is all very well to measure the "length" of a structure, but considerable knowledge of the organism is needed before meaningful landmarks defining "length" can be selected. Defining the measurement is only the first step. There is also the problem of which organism or organisms to measure. Thorough familiarity with the group is necessary before a "typical specimen" or sample can be selected. Take human stature for example. Whom shall we measure? Taxonomically, human beings belong to the species *Homo sapiens* regardless of age, sex, or physiological normality. Nevertheless, since an average figure based on the lengths of all human beings from conception to old age would have little value for any purpose, it is customary to limit the sample to mature males. But what is a mature male? Biologically, human beings are mature at puberty; legally, at arbitrarily set ages from 14 to 21; but growth does not stop until 25 years of age, and height decreases as maturity merges into senility. And which of the mature males are normal? Clearly, selection of the sample to be measured is not a task for amateurs.

There is also the very real problem that not all phenomena are susceptible of measurement. Despite nearly 100 years of study by anthropologists, there is still no satisfactory way to record human eye color or skin pigmentation. And how does one measure an irregular structure, such as the articular surface of a bone? How does one record the shape of an insect's wing? How can one express numerically the relationship of one structure to another, for example, the location of the insertion of a muscle on a bone? How does one measure the length and thickness of a chromosome? Or describe the spiral shell of a mollusk?

Unfortunately, biological structures are rarely simple. Thus it has happened all too often that an apparently simple measurement actually represented the interaction of two, three or a dozen unrelated factors. One example should suffice. From the time of Peter Camper in 1780 until about 1900, anthropologists spent much time devising ways of measuring the facial angle, the angle formed by the intersection of a line from a point between the brow ridges to the lower border of the nose and a line from the lower border of the nose to the ear openings (definitions differ; each anthropologist had his own). The length and depth of the palate, the length of the face and its component segments, the projection of the brain over the eyes, the size of the supraorbital sinuses, and possibly other factors affected the relative positions of the landmarks, and

thus the resultant angle. Anatomically dissimilar skulls could be, and were, described by the same number—a number that meant very little because it summarized too much. Even with carefully selected landmarks, one can never be completely sure of what one is actually measuring.

Once the sample has been selected and the characteristic has been described numerically, it is possible to turn to statistics. Statistical constants offer a convenient way of summarizing the variability in a group. The mean, or average, sums up the central tendency around which the characteristic varies; the mode reflects the value most commonly found; the range describes the extremes of variability in the trait; and the standard deviation provides a means of evaluating the homogeneity of the sample. Once two or more samples have been described in terms of numbers, it is then possible, with the appropriate equations, to compare the samples and to assess the probability of their belonging to the same population. This is the procedure Mather recommends for evaluating experimental results.

The chief use of statistics in biology, beyond the refinement of description through measurement, can be only to confirm or refute conclusions already reached through the observer's knowledge of the groups examined. Considering the number of preliminary, subjective steps, it should be apparent that measurements and statistics can be no more objective than the biologist who uses them. But once the standard deviation has been obtained, there is still the matter of interpretation. The standard deviation may well give a clue to the accuracy of the biologist's judgment in defining the sample measured. A large standard deviation, indicative of considerable variability, may mean either that the trait is more variable than expected or that extraneous elements have been inadvertently included in the sample; a small standard deviation, on the other hand, may be the result of slight variability in the trait or of too rigorous selection of the sample. A reexamination of both the raw data and the specimens may be necessary before proceeding further.

The statistical comparison of two samples is usually based on the mean and standard deviation for a series of traits. Here again it is often a matter of confirming or refuting a prior conclusion. In most cases, an experienced taxonomist (for this sort of comparison is most commonly found in connection with taxonomic problems) will have little doubt about the difference or identity of the samples, for the samples will agree or differ in more than the traits being considered. Yet it is often desired to confirm the diagnosis statistically. When the differences are less obvious to the eye, statistical conclusions are also likely to be ambiguous. The only question to be asked of a statistical comparison is, "Could these two samples have been drawn by chance from the same population?" And the only answer obtainable is one in terms of probability, namely, that the same distribution could have occured by chance in one case in 10, 100, 1000, or 1,000,000. If a given distribution could occur by chance in only one case in 1,000,000, the statistician is perhaps safe in saying that the samples are different; but the interpretation of probabilities at the level of one case in 100, unless based on experience, must be more or less arbitrary. In most cases where a statistically significant difference is found, the morphologic difference is, or should have been, equally obvious without the computations. In cases where the morphologic difference is less obvious, one wonders how much is gained by finding that the statistical difference is not significant.

Comparison of samples on the basis of a single trait, either by statistical or visual means, can seldom lead to valid conclusions regarding the relationship of the groups. Therefore, there have been various methods proposed for the comparison of groups, ranging from a simple listing of the means and standard deviations for each trait in each group to the calculation of a single number derived from all the numerical information available, expressive of the degree of relationship of the two groups. The Coefficient of Racial Likeness, devised by Pearson about 1920, is an example of the latter type of statistical comparison. As described by Morant (*2*), the lower the coefficient between two populations, the more probable that they were random samples of the same population; the higher the coefficient, the less likely that they were random samples of the same population. Accordingly, craniums were measured, and the averages and standard deviation for each measurement were fed into computing machines according to an elaborate formula, and figures expressing racial similarity or dissimilarity were duly obtained. Summarizing the Coefficients of Racial Likeness for some dozen series, Seltzer (*3*) noted that the highest coefficients, that is, the greatest racial differences, were found among three series of English craniums, and that the lowest coefficients, that is, the least racial difference, were found between a group of Tibetans and a group of Congo Negroes. Indeed, by this procedure, some English craniums were found to be more like Chinese or ancient Egyptian craniums than like other English craniums! Seltzer could only conclude that the procedure, which was based on defective biological theory and gave absurd results, was of questionable value. Other coefficients have since been offered to human biologists, but they

have found few takers. The simple list of means and standard deviations, although it appears less erudite, is both far safer and far more informative.

Assume that measurements have been taken and that calculations have confirmed the judgment of the eye that there is only a remote chance that the two samples came from the same population. What then? Having ruled out chance, one must still decide among a host of other factors. Does the observed difference result from sex, age, diet, environment, heredity, or race? Here the statistical calculation of correlation coefficients may provide a clue; but the factors among which correlation is to be calculated must be selected with care in the light of full knowledge of the organism. Or, suppose that measurements were taken and that no difference was found. This still leaves the question of the biological validity of the measurement. Was the measurement pertinent to the problem? (Is increased height necessarily associated with improved diet?) Did a superficial similarity obscure a real difference? (One can obtain the same total length by adding a few long segments or many short segments.)

Finally, it must be emphasized that statistical comparisons of measurements are just that and nothing more. They are comparisons of organisms only to the extent that the measurements selected actually do describe the organisms. Specimens, or series of specimens, vastly different in structure and appearance, may nevertheless coincide remarkably in certain dimensions and indices. All this brings us to two major premises: knowledge of the organism must precede measurement; and measurements, when taken, must be handled with care.

The prediction that descriptive biology must eventually give way to measurement and experiment is unwarranted. The recent rapid growth of the experimental branches of biology and their important contributions to knowledge in no way detract from the importance of the less-glamorous descriptive fields; instead they emphasize the necessity of having adequate qualitative knowledge of the organisms under study. Without such information quantitative studies will, of necessity, proceed in the dark. Statistics can enter the picture only after biologically valid measurements have been obtained. Anatomical structure and embryologic development are the only valid bases for the definition of measurements; and taxonomic information is the only sound basis for the selection of the sample. Taxonomy does not end when a species is given a Latin name and a place in the family tree; the information on many species can and should be extended tremendously over the minimum needed for identification. Measurements may be helpful in making descriptions more precise, and statistical procedures may be helpful in defining the limits of species; indeed, experimentation may provide data pertinent to the description. But regardless of the techniques by which information on organisms is obtained, the majority of biological problems are basically descriptive and comparative. Only when all the qualitative problems have been solved, and this will not be for years to come, can biology justifiably discard description for experiment. And by then natural selection may well have provided new species to describe.

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If one advances confidently in the direction of his dreams, and endeavors to live the life which he has imagined, he will meet with a success unexpected in common hours.
—THOREAU.

Vesalius and the Galenists

M. F. ASHLEY MONTAGU

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IT is a remarkable fact that the two books which are conventionally agreed upon by scholars as marking the end of the Middle Ages and the birth of the new spirit of science were published within one week of each other. The first, *De revolutionibus orbium coelestium* by the Polish canon Nicolaus Copernicus (1473–1543), was published at Nuremberg on 25 May 1543 when the author was 70 years of age; the second, *De humani corporis fabrica libri septem* by the Flemish-born physician and anatomist, Andreas Vesalius (1514–64), was published at Basle on 1 June 1543, when the author was but 28 years of age.

The achievement of Copernicus was, I think, of a higher order than that of Vesalius and considerably more original, yet interestingly enough Copernicus' book was rather more in the tradition of an earlier period than was Vesalius'. Copernicus' book had been eagerly awaited for many years, and when at long last it made its appearance it was well received, but not nearly as well nor as widely acclaimed as was the book of anatomy by the young professor of anatomy at Padua.

The *Fabrica* of Vesalius, a magnificent achievement in every way, was nevertheless on a somewhat more terrestrial intellectual plane than the work of the heavenly Copernicus, a fact that in no way diminishes either the importance or the originality of the work of the younger man. The *Fabrica* represents the culmination of a tendency that was already well developed in the first half of the 16th century. The achievement of Vesalius was to give that tendency its clearest and most complete expression. The *Fabrica* was at once accepted for what it was, representing not so much a break with the Galenic tradition, as has so frequently been stated, as an advance upon it.

It has been the custom to represent the *Fabrica* of Vesalius as having made a complete break with the old traditional anatomy (*1*) and to portray the Galenists as the bad violent men, "headpieces stuffed with straw," while Vesalius is almost invariably depicted as the heroic young genius whom the plodding, leaden Galenists, by their sheer weight, were resolved to destroy.

In the case of Copernicus the Church is cast in the role of villain. In the case of Vesalius the villains have always been the Galenists, with a certain amount of Church thrown in to provide the right *decor*. There is no truth in the first legend, and there does not seem to be any in the second (*2*). In this article (*3*) I should like to do something toward restoring a truer perspective to the view of Vesalius' reception by the Galenists. A great deal has been taken out of context which should never have been removed in the first place. What has thus been wrenched from its context needs to be restored, and only then can the whole be viewed in relationship to the terms and the times to which it properly belongs.

Andreas Vesalius was born at Brussels, the metropolis of Brabant, on the last day of December 1514. His father, of the same name, was court physician to the future Emperor Charles V. His mother, Isabelle Crabbe, seems, so far as the biographers have been able to discover, chiefly remarkable for the fact that she was of English ancestry. Vesalius came from a family that had already produced five generations of physicians, and as the great 18th century anatomists Boerhaave and Albinus say (*4*), "Sprung from such a stock, born of such parents, he felt not impelled to suffer himself to fall away from the ancient virtue of his fathers." The young Vesalius was sent to the military college at Louvain where, among other things, he acquired an excellent knowledge of Latin and Greek, and under the influence of his teacher Guinterius Andernacus developed a burning enthusiasm for the natural sciences. At school he was already depopulating the fauna of his region by dissecting mice, dormice, moles, cats, and dogs. And with the aid of the anatomically inclined Guinterius he succeeded in obtaining a human skeleton by robbing a gibbet.

From school he proceeded to Montpellier, returning on several occasions to continue his studies at Louvain. Meanwhile Guinterius had given up

Greek to teach anatomy at Paris, where another erstwhile teacher of classics, Jacobus Sylvius, was now the most celebrated lecturer on anatomy in Europe.

At the age of 18 Vesalius proceeded to Paris to study medicine under these two men, among others, but more particularly under Sylvius. Here, a devoted pupil, he remained for 3 years.

Whatever the origin of Sylvius' and Guinterius' interest in anatomy may have been, the easiest way to systematize that interest was to begin with the study of the standard textbooks, and then, with their aid, proceed to the dissection of a human body or that of a monkey, a pig, or a bear, each of them in turn if possible. Since the standard textbooks were all Galenic, it is scarcely surprising that these men became confirmed Galenists. They could hardly have done otherwise, for Galen (A.D. 130–200) had a reputation of 1300 years to lean upon, not alone as a great anatomist but as a master of every branch of anatomy and biology. And it must certainly be the judgment of every unbiased student of the history of science that that reputation was well deserved (*5*). There were some who found Galen dull; in Rabelais' copy is written in a bold hand "This Galen is an uncommon dull fellow, a dud, a lump of lead." The truth is, however, that most students of Galen found him rewarding and interesting enough to devote a good many of their student and later years to the study of his writings. His discussions of the systems of the human body are truly remarkable achievements, and he is sufficiently accurate to make it easy to gloss over his errors.

If one did not pay too much attention to detail, and one could hardly do so under the conditions prevailing during the dissection of an unpreserved, uninjected, putrefying body that could not be kept longer than three days, Galen would do very well, especially if one read him in the editions of the learned commentators who had frequently "becommentated" him with such a wealth of matter that fewer words of Galen's than of the commentators were to be found upon each page. There was no anatomy superior to Galen's and under such circumstances the best men could hardly be other than Galenists—which is what Sylvius, Guinterius, and Vesalius were.

It is necessary to insist upon these points in view of the uncritically costive attitude of many modern writers toward any medieval figure who was a follower of Galen. Such writers are themselves guilty of the very sins for which they condemn the Galenists, namely, the uncritical repetition of what has most often been said before, and a lack of acquaintance with the firsthand sources.

Andreas Vesalius, from the first edition of the *Fabrica*.

With the defects and virtues of his classical scholarship to support him and the experience and tradition of anatomy into which he had grown, Sylvius was book-learned and competent in the subject to a degree that earned him a reputation as the greatest anatomist in Europe. As one of his pupils, Loys Vassé, wrote in 1540 (*6*),

> From everywhere flocked to him Germans, English, Spaniards, Italians and others of all nations who all agreed that the like of this admirable and almost divine man was not to be found in the whole of Europe.

Allowing for the ease with which such blossoms bloomed in the verbal springs of the Renaissance, such a eulogy from an old student suggests that there was more in Sylvius than those of a later age would allow who see in him little more than a pretentious, fee-hungry old dunce. In spite of Sylvius' alleged personal unconcern with dissection Vesalius nonetheless always entertained a considerable respect for him.

When Vesalius went to Paris in 1533 to study under Sylvius, the latter had been in that town but a few years. These bare facts require some elaboration, and since we shall be concerned with Vesalius' encounter with what might more properly be called "Galenitis" rather than Galenism through the person of Sylvius, let us inquire what manner of man Sylvius was.

Frontispiece of the *Fasciculo di medicina* (Venice, 1493), by Johannes de Ketham. Petrus De Montagna is reading a lecture *ex cathedra*. The classics of medicine are ranged above to indicate the sources of the lecturer's knowledge: the Greeks, Aristotle, Hippocrates, and Galen; and the Arabs, Avicenna, Haly Abbas, Rhazes, Mesue, and Averroes. On the revolving desk beside the lecturer is Pliny's *Historia Naturalis*; other books are in the cupboard. Peter of Abano's *Conciliator*, the works of Isaac the Jew, and a volume of Avenzoar are on the table below. In the foreground are three sick persons, each with a carrier for a urine bottle.

Born in 1478 in the little village of Louvilly, near Amiens, he was the seventh of 15 children, 11 boys and 4 girls. His father was a poor weaver. An older brother, François, became the principal of the college of Tournay near Paris, and succeeded in entering his brother in that institution. Here young Sylvius exhibited an extraordinary talent for languages, and in the course of several years acquired a thorough knowledge of Latin, Greek, and Hebrew. A French grammar written by him remained for many years a classic. He wrote tolerably good verse, excelled in mathematics, invented machines for transportation by water, and in good time, through the careful perusal of the works of Hippocrates and Galen, developed an intense interest in the structure of the human body. From many passages in his many different works it appears that he took advantage of every chance that came his way for examining bodies: a mason killed by a fall from a roof, a woman dying in childbed, and so on. He was deeply interested in pharmacy and is said to have traveled to distant cities in order to learn new pharmaceutical methods. His poverty was such that he was unable to defray the very appreciable expense of obtaining a medical degree (*7*),

> . . . but his erudition in medical matters was so well known that there gathered about him a considerable number of pupils, and he was so successful that the Faculty of Medicine at Paris forbade him to teach, as it interfered with the regular schools.

It was at this time that Sylvius went to Montpellier where, at the age of 53, he obtained his bachelor's degree on 28 June 1531. When, therefore, he returned to Paris it was as a teacher with an already established reputation in that city. His classes are said to have numbered as many as 400 to 500 students. It was surely not Sylvius' reading of the gospel of Galen that attracted such a vast throng. We know that he did not read particularly well, that his voice was rather harsh and grating, and that his personality, to say the least, was not excessively appealing. Therefore we may be sure that he had something worth hearing to say.

With respect to dissection this was customarily distinctly not a task performed by the professor, but by demonstrators especially employed for the purpose. It should be pointed out that this pattern for teaching anatomy has survived to the present day. The professor will give a course of lectures, or perhaps a few introductory lectures, while the junior members of the department supervise the dissection. If Sylvius ever learned very much about the human body from dissection, the surest way to forget it for all practical purposes was to teach in the *ex cathedra* style that was the custom of his day. But all this does not imply that what Vesalius asserted of Sylvius was true, namely, that he was averse to dissection, for in his little *Introduction to Anatomy* Sylvius says (*8*)

> I would have you look carefully and recognize by eye when you are attending dissections or when you see anyone else who may be better supplied with instruments than yourself. For my judgment is that it is much better that you should learn the manner of cutting by eye and touch than by reading and listening. For reading alone never taught anyone how to sail a ship, to lead an army, nor to compound a medicine, which is done rather by the use of one's own sight and the training of one's own hands. . . .
>
> You would do well to dissect the bodies of those

> who have died of some disease in order that, by recognizing the cause of the malady, you may treat others wisely. Do not dissect only the bodies of men, but also those of monkeys and other animals similar in many respects to man. Yet I will recommend that at first you should work only upon human bodies, thus you will obtain a profound knowledge of the different parts of man which you can apply in dissecting other animals. . . .
>
> Now many do not like at first to view the dissection of man and cannot endure it without great disturbance of mind. Notwithstanding this, they ought, if they can, to accustom themselves from the beginning to look diligently at the body of man while it is being dissected, and then to perform the dissection with their own hands. For this simple manner of learning is the shortest, most certain, and easiest to retain.

These, surely, are not the words of a man with small experience of the dissection of the human body, nor are they, clearly, those of a man who was averse to dissection. The truth is that Sylvius is known to have given a fair number of practical demonstrations on the human body to many of his classes, to the admiration of all those present, including Vesalius.

Among other subjects in which his fame as a teacher was justly deserved was materia medica. Here, as in anatomy, he placed great emphasis on observation and practical work. Long before the establishment of botanical gardens Sylvius was growing medicinal plants, both indigenous and foreign, in the garden of his house just outside Paris in order that his students might learn in the shortest and easiest way, by inspection and observation, the principal characters of these plants.

Study of Sylvius' works reveals that he had at least as good a knowledge of anatomy as any man of his time, that he made a fair number of dissections of the human body, but it is also clear that what he saw, he saw for the most part, through the eyes of Galen. Nevertheless he did make an important and lasting advance upon Galen's terminology. Galen had distinguished the muscles of the arms and legs, for example, by numbers, a notation that was followed by all other authors, including, later, Vesalius. The nomenclature we use today is for the most part, where he named the muscles, Galen's, and where he did not, Sylvius', but not Vesalius'. Sylvius performed the same service for the vessels, and it is the result of the fact that he did not do so for the cranial nerves that we still call them by number today (9). But perhaps his chief anatomical distinction is the fact that he distinguished voluntary from involuntary muscle and described their differences.

Vesalius' judgment of his contemporaries has always been accepted at its face value. Sylvius was no giant, but neither was he a pygmy. He appears to have been a rather austere, avaricious, ill-tempered person, not the perfect preceptor but a teacher from whom one could learn about as much as there was to be learned from the best of the day. Vesalius later disclaimed learning anatomy from him. But as Baker has said (*7*, p. 313),

> Vesalius was in the habit of making derogatory remarks about his preceptors that do not bear close scrutiny. It is incredible that he should have stayed with Sylvius for three years if he had had no opportunity to learn anything while with him. The generally received opinion that Vesalius sprang like Minerva from the head of Jove, armed cap-à pie, and broke the record of all previous ages by dissecting the human body for the first time, does not bear critical examination. Without wishing to detract in any way from his well-merited fame, it seems quite certain that he must have been indebted to his master for a good deal, and that the Foundations of the *Fabrica* were laid in Sylvius' laboratory.

Late in 1536 in the midst of the tumult of the approaching war between France and the Empire, bound by every tie of loyalty to the Emperor Charles V, Vesalius hurriedly returned to Louvain without taking his doctor's degree, although he had fulfilled all the requirements. Less than a year later (6 December 1537) the Senate of Venice had appointed Vesalius, at the age of 22, State Professor of Anatomy. At Padua Vesalius read the anatomical books of Galen three times to his students before he convinced himself that Galen's descriptions frequently failed to agree with his—Vesalius'—own observations on the human body. In 1538 Vesalius was still almost wholly a follower of Galen, as his *Tabulae Sex* show. Indeed, up to this time (*4*, p. 62),

> . . . he used to extol Galen as the second leader of medicine after Hippocrates, as the first and foremost exponent, the coryphaeus, of dissection, a rare marvel of nature and a great admirer of nature, and the author of all good things.

Meanwhile, as his dissections proceeded, Vesalius accumulated so many corrections to Galen that together they formed a fairly large volume. By 1539 he had resolved to write his own anatomy, "if," as he tells us in *The Bloodletting Letter* published in that year (*10*), "the opportunity of bodies offers, and Joannes Stephanus [Van Calcar], outstanding artist of our age, does not refuse his services . . ."

The Bloodletting Letter, which represented Ve-

Title-page of the first edition of the *Fabrica*.

salius' entry into a belatedly discussed controversy, namely, where best to bleed, contains some very definite but very diffidently, even deferentially, made corrections to Galen's description of the veins. Thus, he writes (*10*),

> In support of these views of mine regarding the vein to be lanced in pleurisy, views never previously advanced by anyone, I should endeavour to bring in the oracular utterance of Hippocrates, in the second book on the "Regimen in Acute Diseases," if the authority of Galen did not manifestly contradict me, and I am almost as afraid to argue about his authority as I would be tacitly to doubt the immortality of the soul in our holy religion.

In spite of such protestations of loyalty, Vesalius' defection from the Galenic description of the character of certain veins was obvious.

Vesalius appears to have been perfectly aware of the thinness of the ice over which he was skating, and he was in fact at some pains to avoid situations in which there lay a possibility of giving offense to the more velum-bound Galenists. Thus when the great Giunta edition of Galen's works was being planned, an edition in which the most learned scholars of the day were to participate, Vesalius was invited by Montanus to reedit the large section *De anatomicis administrationibus* earlier translated by Guinterius. He at first declined for fear of giving offense to his former teacher, but he was finally prevailed upon. As Augustinus Gadaldinus, the reviser of the final proofs, tells us in his preface to the volume (*11*):

> It was he who contributed the books on the dissection of the veins, arteries and nerves, with corrections in many places. Persuaded finally by the entreaties and arguments of ourselves and others, to the effect that he would be doing great harm should he let the fear of giving offence to his teacher Andernach—a fear which obsessed him—deprive others of so much that was useful, he also improved the books on dissection to such an extent as almost to make them new.

In this work on the bones Vesalius' corrections of Galen's text are many; except, possibly, for some verbal rumblings from Paris and other places, the work was favorably received. This sort of encouragement served but to reenforce Vesalius' determination to proceed with his own anatomical treatise, all the more so since the talents of an exceptionally distinguished artist, who evidently had no mean knowledge of the human body himself, had become available to him.

The two years from 1540 to July 1542 Vesalius spent working at a feverish pace on his great book. The *Fabrica* contained 663 folio pages including the 278 magnificent woodcuts and numerous decorative woodcut initials. This remarkable *tour-de-force* represented the most original work on the human body published since Galen's writings were first given to the world.

The name of Galen must still be honored and revered as one of the greatest discoverers and systematizers in the history of biology. Galen was of course not himself a Galenist in the worst sense of that word. He was first and foremost an observer and discoverer of nature. Where he erred was in his tendency to extrapolate, to transfer without checking, his findings on one animal to the structure of a totally different one. Thus the details of many structures observed by Galen in pigs, monkeys, and other animals are described as normal structures in man. Human bodies were not usually available to Galen; therefore, with wounded gladiators, monkeys, bears, and pigs, he did what he could. But, unlike most Galenists, he was not one to obtain "base authority from others' books," but deep-searched nature "with saucy looks."

Indeed Galen would have been the first to welcome Vesalius' book. And in truth most of Galen's followers and admirers joyfully welcomed Vesalius' book from the day of its appearance. They were

perfectly willing to accept Vesalius' corrections of Galen even when Vesalius was wrong and Galen was right. It is sufficient to mention the names, among these, of Fallopius, Aeccius, von Veltwiyck, Roelants, Gesner, Cornarius, Fuchs, Henerus, and Paré. There was no battle for the acceptance of the new anatomy and the new method of observation; both were accepted at once.

Who could help but be overwhelmed by this glorious work? Alas, the sort of people we should expect. At first, the ungenerous and the poor in spirit, persons who, like Sylvius, had been planning an anatomy of their own, and had now had all the underpinning knocked away from under them by this unfledged usurper of their authority. For what, indeed, should a bold and impetuous upstart of 28 years be doing writing such books challenging thus not alone their own but Galen's authority? There is a point beyond which impetuosity becomes madness. And this is, in fact, what Sylvius said of Vesalius.

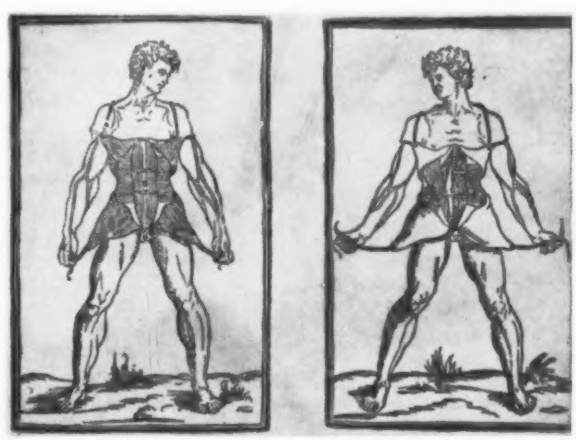
Then there were those who were biliously jealous, the haters of perfection of work and personality in the workman; there were also the choleric and dyspeptic, and the avowed and unavowed enemies and detractors. Under the cloak of strict adherence to the evangel of Galen to which the generations and time had given the imprimatur of unchallengeable truth, these combined forces became that skulking group of bigots whom it has been the custom to identify as "the Galenists." But the point it is highly desirable to establish is that they were not Galenists, but bigots, and that most of them were so not because they were the children of their time, but because, for purely personal reasons, they did not like Vesalius, and they were going to tell the reason why—even though it was not the true one.

In the past there has been a strong tendency to romanticize Vesalius and to represent him as something of a martyr who was eventually hounded to death by "the Galenists," those phlogistonists of anatomy who resented the attack upon their doctrine, and who

> Fixed in their groove,
> By apostolic knocks
> Proceeded to prove
> Their doctrine orthodox

by anatomizing Vesalius in barbed words. The ingredients, indeed, are all there for the purposes of the writer of romantic history (*12*) or the occasional medical essayist. The serious student appraising the facts views the evidence somewhat differently, and, I think, more justly. He sees that the few stupid attacks that were made upon Vesalius a fair number of years after the publication of the *Fabrica* by those who disliked him personally, have been dissociated from particular persons and generalized for the age in which Vesalius lived. The notion which it has been sought to convey is that the living fossils of the Middle Ages who had lingered on into the Renaissance were in spirit antipathetic to Vesalius' way of thinking, that is, reliance upon observation rather than upon authority, that Vesalius' *Fabrica* came upon the last or lingering phases of those Ages as a bolt from the blue, and that the *Fabrica* represented a clean break with the existing tradition of anatomical knowledge and teaching, and that for all these "obvious" reasons, as well as Vesalius' alleged bumptiousness, the opposition to Vesalius and his work was great. This conventionalized story does not agree with the facts.

As Lynn Thorndike has indicated (*13*), the movement among anatomists of the first half of the 16th century was, in general, definitely oriented in the direction of greater reliance upon observation and upon rejection of Galen when he was in disagreement with the observed facts. Thus Jacapo Berengario da Carpi (about 1470–1530), professor of surgery at Pavia and Bologna, dissected more than 100 human bodies and was the first to introduce anatomical figures into an anatomical text. In his popular commentary upon the medieval handbook of Mundinus, published in 1521 (*14*), and which as late as 1664 was translated into English (*15*), he decries the habit of anatomical writers who follow authorities like cattle and warns the reader not to be deceived "by some of our moderns who involve anatomy with authorities and not with observation." He says also that he always accepts Galen's views except where observation is at discord with them (*14*, fols. 240r, 398r, fol. 412v).



The abdominal muscles, from Jacapo Berengario da Carpi, *Isagogae breves* (Bologna, 1522 and 1523).

In keeping with the humanist tradition of the age, Carpi's textual criticisms were minute and extensive. Fallopius not unjustly called him the first restorer of anatomy. A careful study of his work serves to show to what an appreciable extent a man like Carpi paved the way for Vesalius.

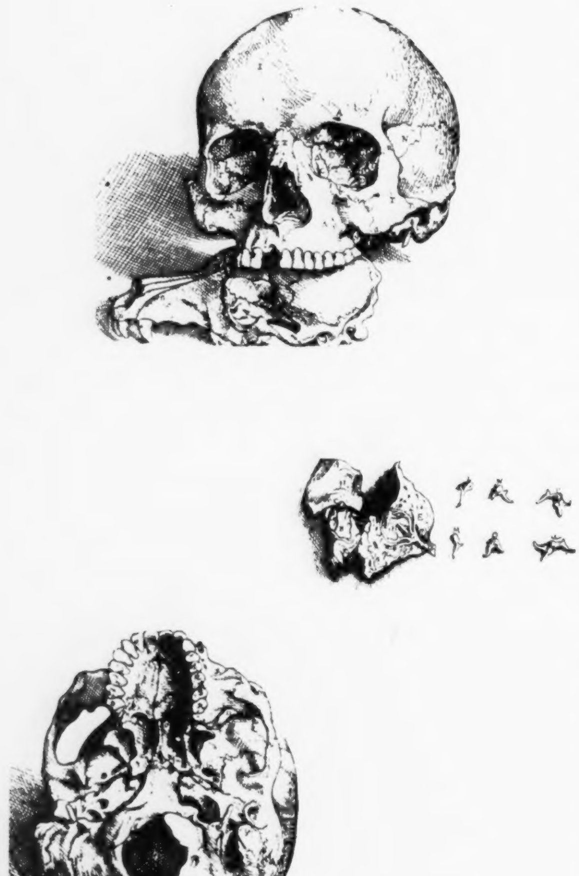
A work transitional between that of Carpi and that of Vesalius is represented by the *Liber Introductorius Anatomiae* of Nicolaus Massa (d. 1569), which was published at Venice in November 1536 and dedicated to Pope Paul III (*16*). Massa criticizes the anatomists of his day for writing so much that is not essential to the science of dissection, for their sophistic arguments and distinctions that so befog the minds of youthful students that they fail to grasp anatomy itself (*16*, fols. 3v, 15v), "moderns who know things only by name," and who (*16*, fol. 10r) "by trusting in the dialectica and questions current in the schools have failed to observe." In his little volume Massa describes only what can be seen in a single dissection and is most essential in medicine; he omits all discussion of late medieval questions to which Carpi gave so much space, and insists upon the importance of frequent dissection instead of disputations and the use of authorities. Furthermore, he provides the reader with a fair number of new observations based entirely upon his own dissections.

Both Carpi's and Massa's books were widely read and approved, and there can be little doubt that Vesalius had read and been influenced by them. At any rate, these works clearly reflect the advancing new spirit of the age and show beyond question that two of the most widely read forerunners of Vesalius in anatomy were far from being blind followers of Galen, that they were quite independent and critical in their judgments, and that they placed considerable stress upon the need for dissection and the importance of direct observation.

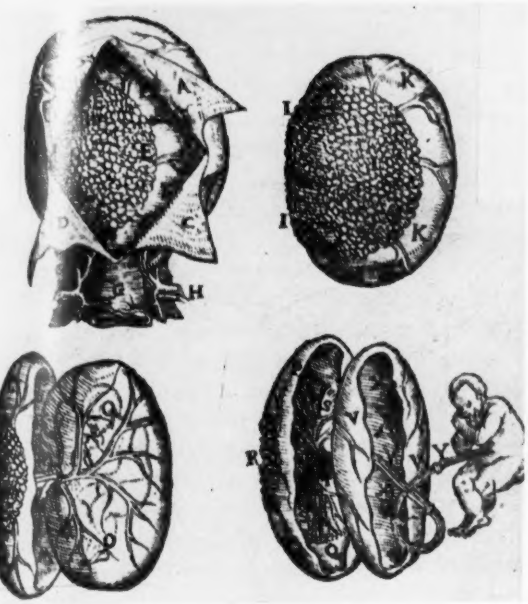
In his *Fabrica* Vesalius simply continued in the spirit that was the best of his time, in the direction set by such men as Carpi and Massa. The age had been prepared and was ready for such a work as Vesalius'. When it appeared it was welcomed as that complete presentation of the facts to which earlier introductory volumes, including one by Vesalius, had led the way. It was not a bolt from the blue but a work which it had practically been expected that someone would write, and of a kind which several men, including Massa, had hoped to write. As for the alleged break with the Galenic tradition this was in point of fact no more radical than was that of Carpi or Massa.

Vesalius did on a grand scale what they had done on a lesser one. Being a young man of some means, he was able to command the services of one of the best artists of his day and of the best printers of his time to produce a folio volume that, even if the text had not been as good as it was, would nevertheless have established itself instantly in the esteem of all students of medicine.

Although he often criticizes Galen, sometimes not altogether justly, he presents the subject of anatomy in seven books in the traditional Galenic order, commencing with the bones, then proceeding to the ligaments and muscles, and so on. Indeed Vesalius is at pains to express his admiration for Galen, whom he calls "after Hippocrates the prince of medicine . . . easily among the foremost of the teachers of anatomy," and while correcting his errors he criticizes "those who today call themselves Galenists but do not study anatomy and dissect as Galen did." (*17*). The *Fabrica* actually reproduces



Illustrations from the *Fabrica*. Top: the human skull compared with that of a dog to show the absence of the premaxilla on the facial aspect of the human skull and its presence in the dog. Middle: the petrous portion of the temporal bone with different views of the auditory ossicles. Bottom: base of the human skull.



The uterus, placenta, and fetus. From the *Fabrica.*

many Galenic errors both in the text and in the illustrations, and reveals the shallowness of Vesalius' knowledge of certain obvious parts of the human body as well as his reliance upon Galen (*1*).

What would seem to be Vesalius' almost deliberate neglect of more than a passing reference to contemporary anatomists who, like Carpi and Massa (so far as I can discover the latter was not mentioned at all) were the immediate intellectual forerunners of Vesalius, and his rather overaggressive preface in which he criticizes "the supineness of the medical profession," and gives a somewhat critical picture of the state of medicine in his own day, undoubtedly annoyed some people and amused others. Most agreed that its faults were few and its virtues many, that, in short, it was a splendid book. As Fallopius put it, what Carpi had begun Vesalius perfected (*18*).

In his preface, without naming names, Vesalius made several pointed references which hit home hard. No one could fail to recognize Jacobus Sylvius as the object of these remarks. Speaking of his attempt to restore anatomy to some degree of completeness Vesalius writes (*19*):

> But this effort could by no manner of means have succeeded, if, when I was studying medicine at Paris, I had not myself applied my hand to this business, but had acquiesced in the casual and superficial display to me and my fellow-students by certain barbers of a few organs at one or two public dissections. For in such a perfunctory manner was anatomy then treated in the place where we have lived to see medicine happily reborn . . . except for eight muscles of the abdomen, disgracefully mangled and in the wrong order, no one (I speak the simple truth) ever demonstrated to me any single muscle, or any single bone, much less the network of nerves, veins and arteries.

Referring to his illustrations, Vesalius writes (*19*, p. 1364)

> But there comes into my mind the judgment of certain men who vehemently condemn the practice of setting before the eyes of students, as we do with the parts of plants, delineations, be they never so accurate, of the parts of the human body . . .

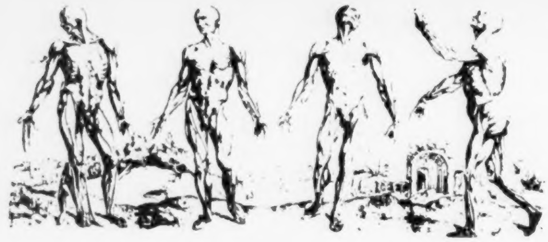
Sylvius had a well-known prejudice against anatomical delineations, and especially inveighed against those of Carpi, saying (*20*) that they could "at best, only serve to gratify the eyes of silly women."

Finally, Vesalius delivered himself of this seraphic blast (*19*, p. 1364).

> Moreover I am aware [first] how little authority my efforts will carry by reason of my youth (I am still in my twenty-eighth year); and [second] how little, on account of the frequency with which I draw attention to the falsity of Galen's pronouncements, I shall be sheltered from the attacks of those who have not—as I have done in the schools of Italy—applied themselves earnestly to anatomy, and who, being now old men devoured by envy at the true discoveries of youths, will be ashamed, together with all the other sectaries of Galen, that they have been hitherto so purblind failing to notice what I now set forth, yet arrogating to themselves a mighty reputation in art.

Whether or not Vesalius had Sylvius in mind when he penned these words, it is hardly a precarious conjecture to suppose that Sylvius felt himself grievously offended; but apart from adversely criticizing Vesalius' contributions to anatomy in his classes he held his peace. The picture generally conveyed is quite otherwise, Sylvius being cast in the role of mephitic leader of those who "almost immediately" after the publication of the *Fabrica* rushed to the attack upon it and its author. (*21*) That "almost immediately" lasted 8 years, Sylvius publishing nothing until 1551, when he was provoked into doing so partially by the publication of the details of a private correspondence between them which Vesalius had initiated. This publication took the form of a rather long letter replying to certain queries of Joachim Roelants concerning the china root. The original epistle is dated 13 June 1546 and it was published late in 1546 (*11*, p. 163).

Here Vesalius reveals that some time after the publication of the *Fabrica* he had written Sylvius with the greatest respect and deference, saying that



Some of the "muscle-men" from the *Fabrica.*

if there were any comments in his books on anatomy which had displeased Sylvius, he, Vesalius, hoped that Sylvius would tell him what they were. To this Sylvius made answer at length to the effect that he could not believe Galen was in error and that friendship between himself and Vesalius was possible only if the latter made a complete retraction of his criticisms, which, he added, were probably the result of his youth and too-long association with the Italians. Vesalius replied that he was unable to oblige since age and experience had but served to confirm his views. He writes (*22*):

> Many persons are hostile to me because in my writings I seem to hold in contempt the authority of Galen, the prince of physicians and preceptor of us all, because I do not agree indiscriminately with all his opinions, and especially because I have demonstrated that some errors are discernible in his books. Surely scant justice to me and to our studies and indeed to our times! . . . I would rather have counted in this class any one than Jacobus Sylvius, an ornament to the physicians of our age; but from the letter sent by your son, in which he disclosed that he had read my books, he proved very decidedly that he *does* belong to this group. So now you can easily deduce what ground I covered in my letter to him which I sent on to you from Nimwegen so that your son could deliver it to Sylvius.

It is a thousand pities that a copy of the letter mentioned has not survived, but we may well guess its contents, and we may be sure that Sylvius thoroughly disapproved them.

In 1551, when Sylvius was 72 years of age, he published a broadside aimed against Vesalius entitled *Depulsio Vesani cuiusdam calumniarum in Hippocratis et rem anatomicam* (The Refutation of the Calumnies of Vesanus). The punning change of Vesalius into Vesanus (madman) was, as Kingsley has said (*12*)

> but a fair and gentle stroke for a polemic, in days in which those who could not kill their enemies with steel and powder, held themselves justified in doing so, if possible, by vituperation, calumny, and every engine of moral torture.

Sylvius' absurdly fantastic arguments brought forward to show that Galen had indeed dissected and described the human body, and not that of monkeys, as Vesalius had asserted, convinced nobody, but they made a great many people laugh—at Sylvius.

When, 20 years after the publication of the *Fabrica* the sycophantic Franciscus Puteus published a scurrilous attack upon Vesalius (*23*), one of the persons Puteus called upon to testify to the soundness of his criticism was Gabriel Cuneus. Cuneus, who was professor of anatomy at Pavia 1554–74, at once made it thoroughly and unequivocally clear that he was on the side of Vesalius. His reply to Puteus, published in 1564, was crushing and definitive, and put an end to the last of the detractors of Vesalius (*24*).

Obviously a number of people were jealous of Vesalius' achievement and hostile toward him on that account, and some also because his self-conceit annoyed them. Certain physicians of the Emperor Charles, elderly colleagues of Vesalius, attempted to injure his standing with the king (*25*). The resentment, however, was a personal one, directed against the objectionable youth and imperiousness of its object. It was not a school of thought which through its followers was hostile to Vesalius but frustrated individuals who seized upon Vesalius' criticism of Galen as a peg upon which to hang their abuse of the critic. Vesalius was himself a Galenist, and, apart from a few bigots, the Galenists were with Vesalius from the first. In the past the personal enmity of a comparatively few men has been made to appear as if it were the reflection of the spirit of the time, the Middle Ages in the person of the Galenists against the Renaissance in the person of Vesalius.

I have attempted to indicate that this view is not correct.

References and Notes

1. How untrue this is has been shown by C. Singer, "Some Galenic and animal sources of Vesalius," *J. Hist. Med. Allied Sci.* **1,** 6 (1946).
2. C. D. O'Malley, "Andreas Vesalius' Pilgrimage," *Isis,* **45,** 138 (1954).
3. This article is a revised version of an unillustrated article of the same title contributed to the *Festschrift* for Charles Singer, *Science Medicine and History,* E. A. Underwood, Ed. (Oxford Univ. Press, New York and London, 1953). Thanks are due to the editor of that magnificent work and to the Oxford University Press for permission to reprint.
4. H. Boerhaave and B. S. Albinus, "The life and work of Andreas Vesalius." (Translated by Benjamin Farrington from the preface to their edition of Vesalius' works, *Opera Omnia Anatomica et Chirurgica,* Leiden, 1725). *Trans. Roy. Soc. S. Africa* **19,** 49 (1930).
5. G. Sarton, *Galen of Pergamon* (Univ. of Kansas Press, Lawrence, 1954).

6. L. ...mmer and J. B. de C. M. Saunders, "The a... ...nical compendium of Loys Vassé (1540)," A... Med. History ser. 3, 1, 351 (1939).
7. F. ...ker, "The two Sylviuses. An historical study." B... Johns Hopkins Hosp. 20, 329 (1909).
8. O... Omnia Jacobi Sylvii (1635) p. 127. The t... ...lation is by Frank Baker (7).
9. S... ...us did not give his or any other name to certain st... ...tures commonly referred to him but which b... ...ng rather to an anatomist of the 17th century w... whom he is often confused, namely, François d... ...a Boë (1614–72), professor of practical medicine at Leiden, who was also known by his Latin cogno... ...n as Sylvius. It was Franciscus and not Jacobus who gave the really first adequate anatomical de... ...ription of the brain and sinuses of the dura mater, and it is after Franciscus that the fissure of Sylvius is named, as is also the aqueduct, which was, in fact discovered long before Sylvius was born.
10. "Andreas Vesaluis Bruxellensis: The Bloodletting Letter of 1539." Translated by J. B. de C. M. Saunders and C. D. O'Malley. In M. F. Ashley Montagu, Ed., *Essays and Studies in the History of Science and Learning Presented to Dr. George Sarton on His Sixtieth Birthday* (Schuman, New York, 1946,) pp. 3–74.
11. *Galeni Omnia Opera* (Venice, 1541). Quoted from H. Cushing, *A Bio-Bibliography of Andreas Vesalius.* (Schuman, New York, 1943).
12. See for example Charles Kingsley's wholly delightful but thoroughly euhemeristic piece, "Andreas Vesalius" in his book *Health and Education,* (London, 1887), pp. 385–411.
13. L. Thorndike, *A History of Magic and Experimental Science* (New York, 1941), vol. 5, pp. 498–531.
14. *Carpi Commentaria cum amplissimis Additionibus super anatomia Mundini una cum textu eiusdem in pristinum et verum nitorem redacto* (Bononia, 1521).
15. Μικροκοσμογραφια *or, a description of the body of man; being a practical anatomy, shewing the manner of anatomizing from part to part. . . . Done into English by H. Jackson* (London, 1664).
16. N. Massa, *Liber introductorius anatomiae sive dissectionis corporis humani nunc primum ab ipso auctore in lucem editus* . . . (Venetiis, 1536) fol. 10r.
17. A. Vesalius, *De Humani Corporis Fabrica Libri Septem* (Basileae, 1543), Bk. ii, fol. 58, fol. 234–40, 342 ff.
18. G. Falloppii, *Observationes Anatomicae* (Venetiis, 1561), fol. 25r.
19. Benjamin Farrington (Translator), "The preface of Andreas Vesalius to *De Fabrica Corporis Humani* 1543." *Proc. Roy. Soc. Medicine* 25, 1337 (1932).
20. "They can at best only serve to gratify the eyes of silly women [*mulierculis oculos pasturis*], to the true physician they must always be a hindrance, for it is his duty to view and to handle the body as a whole and in all its parts, becoming acquainted with the substance, size, number, shape, situation and connections of each as far as the fingers can reach, not confining this examination to the surface, the only portion that can be represented in pictures." "Ordo et ordinis ratio in legendis Hippocratis et Galeni libris." *Opera Medica Jacobi Sylvii* (1635) fol. 5.
21. See, for example, the excellent lecture by Arturo Castiglioni, "The attack of Franciscus Puteus on Andreas Vesalius and the defence by Gabriel Cuneus," delivered at the 400th anniversary celebration of the publication of *De Humani Corporis Fabrica Libri Septem* (The Historical Library, Yale Univ. School of Medicine, 1943) p. 35.
22. *Epistola rationem modumque propinandi radicis Chynae decocti* . . . (Basileae, 1546). See also Cushing (*11*) p. 162.
23. F. Puteus, *Apologia in anatome pro Galeno, contra Andream Vesalium Bruxellensem* (Venetiis, 1562).
24. G. Cunei, *Apologiae Francisci Putei pro Galeno in Anatome, examen* (Venetiis, 1564).
25. C. D. O'Malley and J. B. de C. M. Saunders, "Andreas Vesalius Imperial Physician," in E. A. Underwood, Ed., *Science, Medicine and History,* Oxford Univ. Press, London and New York, 1953) vol. 1, pp. 386–400.

Experiment, directed by the disciplined imagination either of an individual or, better still, of a group of individuals of varied mental outlook, is able to achieve results which far transcend the imagination alone of the greatest philosopher.—LORD RUTHERFORD.

National Defense against Atomic Attack

WALTER E. TODD, WILLARD S. PAUL, VAL PETERSON

Major General Todd is a career air officer who served as deputy chief of staff for operations of the Eighth Air Force in the United Kingdom during World War II and later as military air attaché at the American Embassy in Moscow. Between 1949 and 1952 he served as assistant deputy chief of staff for operations and now is the commanding general of the Western Air Defense Force. Lieutenant General Paul served as commander of the 25th Infantry Division in World War II. He is a specialist on problems of economic mobilization for defense and is assistant to the director for plans and readiness of the Office of Defense Mobilization. Mr. Peterson was governor of Nebraska from 1947 to 1953. After serving as an administrative assistant to the President of the United States, he became the administrator of the Federal Civil Defense Administration. This article is based on papers presented at the joint symposium of the National Academy of Economics and Political Science; Section K, AAAS; and the National Social Science Honor Society, Pi Gamma Mu, at the meeting of the AAAS, Berkeley, California, 26–31 December 1954; edited by Donald P. Ray, executive secretary, National Academy of Economics and Political Science, and secretary, Section K, AAAS.

Air Defense and the National Safety

THE primary responsibility for the defense of America has been charged to the United States Air Force. To enable the most effective employment of all air defense weapons, the Secretary of Defense, in September 1954, approved the activation of the Continental Air Defense Command, composed of the Air Defense Command of the Air Force, the Army Antiaircraft Command, and those Navy and Marine forces in the continental United States that can contribute to the air defense mission.

The Continental Air Defense Command is aided in its defense of the United States by the Strategic Air Command. It has an important defense responsibility in the sense that the capability of the United States for decisive retaliation certainly discourages aggression. Even the existence of a powerful offensive force, however, does not necessarily insure security from attack, nor can the Strategic Air arm prevent enemy bombers from flying to their targets once they are air-borne.

The basic objective of both the Strategic Air Command and the Continental Air Defense Command is to become so strong and so effective that an enemy could not afford to put these two air arms to the test. Secretary of the Air Force Talbott stated recently in an address before the National Press Club: "You exist if you are too tough to tackle. You perish if you are weak or unready."

Another objective of the Continental Air Defense Command, in the event of hostilities, is to be able to destroy or divert as many approaching bombers as possible before they reach their targets and, in so doing, protect all those things we regard as dear to us—our families, our homes, our industry—in other words, our way of life.

In order to attain these objectives, the air defense elements must reach the maximum capability of detecting enemy aircraft, identifying them, intercepting them, and destroying them. Of course, what makes this so difficult is that technologic advances in air defense are countered by a corresponding growth in enemy offensive strength. As development in jet interceptors increases, for example, the enemy keeps pace in development of jet bombers. The matter becomes, therefore, that of a technologic race for the highest stakes the world has ever known. Whether or not this race can be won depends for the most part on scientists and their scientific potential. The continuation of the American way of life depends predominantly and decidedly on the fulfillment of future air defense requirements.

In order to detect approaching aircraft, radar sites must be placed strategically for greatest effectiveness. Such radar sites dot the American coastline and the northern borders and extend across Canada and Alaska. Building radar nets is a long and costly process. From the start, budget and production limitations necessitated beginning the radar coverage near a few vital targets and subsequently

extending the coverage outward. The outward extension, of course, had to be confined to the most logical penetration routes. Among these logical routes are the sea and polar approaches. Ground radar sites, therefore, are located to guard these approaches. To provide extended early warning, some radar units are being located well out to sea on Navy picket ships and on "Texas Tower" platforms built on wave-covered reefs. There are also air-borne early warning radar devices—literally "flying radar stations," which will be capable of sweeping the skies far out over the ocean beyond the Navy picket ships and "Texas Towers."

Even with this expansion, radar is not plentiful enough to detect all approaching aircraft. Then, too, existing radar has its operational limitations, for example, the inability to penetrate hills and mountains; the inability to project its waves around the curvature of the earth; and the inability to distinguish, with consistency, the types and numbers of aircraft. It is, therefore, essential that there be maintained a civilian ground observer program to cover these weaknesses in our detection system. The civilian volunteer members of this corps deserve a resounding tribute for the tremendous service they have already devoted to the security of the United States.

The problem of identification of aircraft has been most difficult to resolve, and will continue to be, without unduly restricting the normal flow of air traffic. There are two methods of identifying aircraft that are detected by radar or by the Ground Observer Corps—one is correlation of the track of an aircraft with known flight plans; the other is the sending of interceptor aircraft to make visual identification.

Many steps have been taken and more are being formulated to reduce the number of aircraft that require visual identification by fighters. The Civil Aeronautics Administration has established areas known as Air Defense Identification Zones along our borders and over certain critical interior areas. The pilots of all aircraft penetrating such zones must file detailed flight plans before initiating such flights. These flight plans are then processed immediately through CAA and Air Defense channels, so that detected aircraft can be associated with such intended flights with respect to time, course, altitude, and air speed. If aircraft conform to these flight plans or promptly advise the proper ground station of changes, then the identification problem is simple. However, if pilots fail to file flight plans or do not adhere to those submitted, then fighters must be sent out for purposes of identification. This is costly and, of course, not particularly well received by aircraft crews and passengers. It is, nevertheless, mandatory in order to eliminate the possibility of an unidentified aircraft being the first of an aggressor force to arrive within our radar surveillance.

To accomplish interception, the Air Force has been allocated the finest and fastest jet interceptors presently available. Among the interceptors is the single-seater F-86D, an advanced version of the famous Sabre Jet, which proved its prowess in the Korean conflict. There are also the F-94C Starfire and the F-89D Scorpion—each carrying a two-man crew, pilot, and radar observer. All of these aircraft have their own air-borne radar and are capable of intercepting a target in any weather that might be encountered day or night. Once aloft, these interceptors are virtually steered to the vicinity of the target by ground radar controllers located at either land, naval, or air-borne stations. As the interceptor reaches the area of the unidentified plane, the aircraft's own radar takes over and guides it to the target.

Augmenting the all-weather interceptors to meet emergencies are other types of fighter aircraft from the Tactical, Strategic, and Training Commands of the Air Force, the Air National Guard, the Navy, and the Marine Corps. The augmentation fighters are armed, for the most part, with machine guns, whereas all-weather interceptors carry more lethal weapons—air-to-air rockets that are automatically aimed and fired by radar. Enemy planes that elude fighter interceptors become the targets of the Army Anti-aircraft Command, supported in coastal areas by shipboard naval firepower. The Army units of the Continental Air Defense Command encircle vital target areas and are presently completing their conversion to the "Nike" guided-missile weapon. The Army antiaircraft units are of the nature of "last-ditch" defense.

The success in integrating tools and weapons of air defense into an effective combat force depends entirely on communications. Communications and electronics are inseparable twins, for without one the other is of little value. Landline telephone and teletype are the primary means of communications. Radio constitutes an alternate method of communications when wire facilities become disrupted or overloaded. For the purpose of contacting and controlling aircraft in flight, however, radio is the primary and only means of communication. The many radio frequencies are used in a variety of ways to maintain this necessary contact.

A very recent and gratifying development that illustrates the cooperation on the part of governmental and civilian agencies in the communications field is known as Project Broadcast Fighter Control. Very shortly each radar station in the United

States that controls fighters to effect interceptions will be tied in by landline to the most powerful commercial broadcast station in its vicinity. By this means, control messages can be relayed for transmittal without delay over such broadcast stations, should this become necessary in an emergency. This arrangement involves the Air Force, the Federal Communications Commission, the radio industry, and, of course, all the broadcast stations and networks concerned. It is a significant step forward.

An enemy attack, striking tonight or tomorrow, might cause several million American deaths. A high degree of readiness requires around-the-clock alertness of the entire air defense system. The disastrous consequences of a sneak attack must be avoided. As improvements are made in detection and warning capability in every conceivable way, the loss of American lives will be reduced manyfold. For best results, the enemy would most likely plan to execute the sneak type of attack when a guard is down. This must never be permitted. Awareness by the public to threat of military aggression fluctuates with the international situation as it is presented to them. Military readiness cannot be altered as rapidly, or to the same degree, as changes occur in the international situation. The degree of military readiness and alertness must be keyed to the capability—not necessarily the probable intentions—of the enemy long-range air force. Enemy leaders do not vary the strength of their military establishment as they vary their complex, but determined, foreign policy. As long as weapons of terrific destruction and the means for delivering them on the United States remain in the possession of a potential enemy, this country cannot permit anything in the Continental Air Defense Command that will risk the survival of a free nation.

The over-all capability of completing all the processes described here must be maintained, employing the weapons associated with them, within a matter of a few minutes—that is, from the detection and identification of the first hostile aircraft until the outcome of the first nuclear weapon attack on this country is known. Work proceeds vigorously, by every means possible, to improve warning, which is the most practical means of increasing the afore-mentioned span of time. Other developments and advances will affect only an actual air battle itself.

In order that the United States may have a near-perfect air defense, capable of destroying any aerial armada an enemy might launch, there is needed a far more extensive and effective air defense system than exists at present. Ideal requirements for such a force are probably not beyond scientific and productive capabilities. Within what is believed to be practical limitations, certain required advances may be indicated.

Several rings of radar stations capable of detecting aircraft or missiles at least 80,000 ft in altitude are needed—completely encircling target areas and the greater part of the North American continent. These stations should have improved capabilities for detection in both distance and altitude. They must operate continuously with a minimum of maintenance and have the capability of performing effectively in spite of enemy electronic countermeasures. There are bound to be low-level terrain gaps in these radar nets that will have to be filled—wherever those gaps might be—by about 1 million civilian Ground Observer Corps volunteers, almost 3 times the number at present. All of these requirements are in presently approved programs. There must be all-weather interceptors that can fly considerably faster and higher than any attacking bomber, and they must be able to reach that bomber in a matter of minutes. Furthermore, these interceptors must be stationed in strategic locations and have a combat radius that will enable them to engage enemy aircraft in combat as far from continental borders as possible after radar detection. They must be designed simply so that they can be flown and utilized in combat by young second lieutenant pilots of limited flying experience. These fighter aircraft must be available in sufficient quantity to cope with the largest possible number of aircraft the enemy might employ.

For armament, firepower that "cannot miss" is needed, both in air-to-air and surface-to-air weapons. A greater range in our surface-to-air weapons is also necessary so that crashing bombers that explode their nuclear cargoes will do so in sparsely populated or unpopulated areas, where radioactive fallout will cause the least amount of harm.

The communications system must be able to render adequate performance at all times. In addition to the tremendous number of landlines required, radio communications will continue to be essential. The quality, range, and reliability of transmission must be perfected on an already overcrowded frequency spectrum. Furthermore, the communications system must not be vulnerable to sabotage and interference.

Last, in order to achieve a completely effective air defense, it is necessary to consider the human element. In present radar operations, men must study the sweeping, moving lines on radarscopes. This is undertaken for considerable periods. It might be compared to watching the sweep second hand on a clock for hours at a time without looking away. When men become weary they make

mistakes—mistakes that can be costly. Any reliable device that can be developed to replace human functions should be installed in the air defense system. By doing so, a source of possible human error and time delay may be eliminated, both of which are detrimental to air defense operations. A great deal of support is needed from research and development in this field.

Perhaps, the most important requirement where the human element is concerned is this: there must be stability of trained and experienced people. Sufficient incentive must be offered to induce trained electronics personnel and skilled fliers, as well as many others, to remain in the Air Force.

If an assumption were made of a perfected air defense system, such could be called immediately into action. This vast and effective defense force would meet today's threat. But even assuming such a wonderful mechanism, complacency still could not be permitted. Planning must be long-range, and technologic advances, both offensive and defensive, must outpace those of a potential enemy. If superiority in this respect can be obtained, the glorious reward would be that no nation would dare to attack us.

Many of the items presently needed for contemporary air defense are already well on the way, from "prototype to production type." In research, scientists have made startling progress in the past decade, but the problems encountered are staggering even to the imagination. A push-button defense, for example, would involve the automatically launched and steered defensive guided missile with a computer-type automatic pilot. Such a missile must be capable of speeds many times the speed of sound and have an altitude range into the stratosphere. It must also be unsusceptible to electronic countermeasures, and its power plant and automatic guidance must be such as to provide precise accuracy at the target. The complexities of missile warfare are multitudinous and difficult to comprehend. The concept can be likened to trying to hit with a rifle bullet the bullet just fired from another rifle. Until a completely effective pilotless aircraft or missile can be acquired, the most difficult barrier to transcend is the "human barrier." As long as an aircraft must be manned, its performance must, of necessity, be limited by what the human body can endure. Many problems in this area are extant today.

An adequate air defense for the nation cannot be achieved by the military alone. It requires a high degree of cooperation from men of science, from industrialists, and from many other segments of the population. The courage, the perseverance, and the foresight to achieve the means needed to defend our country will require the efforts of all American citizens.

Security Measures for the National Economy

The necessity to plan for the defense of the national economic system has been precipitated by the phobia of the thermonuclear weapon. Enemy possession of atomic and hydrogen bombs, deliverable at random objectives within territorial borders, poses the great difference in defense planning from such experiences of World War II. This vital fact cannot be disregarded; to do so is to invite national calamity. To the commonly discussed threat of aerial attack must be added the possibility of submarine and clandestine attacks with nuclear, biological, or chemical weapons.

Total war can have only one adequate defense: total preparation. Total preparation, no matter how well conceived, is of doubtful value unless it is *ready* for an attack. An enemy, moreover, does not have to destroy every city in the United States to cause an unacceptable amount of damage. Just 20 of our largest metropolitan areas contain 56 percent of the defense industrial capacity and 30 percent of the population, including 43 percent of the industrial working force. Two-thirds of the industrial capacity, along with one-third of the total population and nearly 7 million industrial workers, is located in 31 metropolitan areas. Here, then, is the volume of concentration that makes the nation vulnerable. Added to these figures is the fact that many of the most essential functions of Federal, state, and local governments are located in these same few target areas. A clear picture, thus, may be had of what a famous American scientist has termed "our industrial-governmental glass jaw."

What can be done about this condition? There are those who would rely on normal growth of the economy. They point out that this accounts for both doubling and rebuilding the entire productive machinery approximately every 40 years. From a purely theoretical standpoint, it is possible to reduce the target value of production centers about 5 percent each year just by making certain that all new industrial facilities are properly dispersed. But is there sufficient time to accomplish total reduction of industrial vulnerability by this means? It is possible to improve the structural strength of facilities so that the area of major damage caused by any one bomb could be reduced by 32 percent, with an increase in construction cost ranging from 10 to 20 percent. Can this, solely, be regarded as an acceptable measure for national security?

Certain imponderables must be faced. Who knows when an attack may occur, by what specific means, and at what specific locations? Is prepara-

tion necessary for one or two H-bombs, a series of A-bombs, nerve or germ warfare—or all of these? How closely can administrative and industrial defense plans, evolved through interagency and intergovernmental conferences, follow the technologic changes that may create new and startling threats almost overnight? How can advance preparation be accomplished when much of it relies upon administrative mechanisms dependent upon new laws—laws that no peacetime Congress or peace-loving people can reasonably be expected to enact? What will be the relative effect of Federal budget allocation among the military, research, industrial mobilization, and nonmilitary defense agencies in terms of the ability of the country to absorb blows and still be able to launch effective military retaliation in sufficient *quantity* and over sufficient *time* to win through to whatever "victory" can be gained?

These are not easy questions to answer in a republic whose entire history has been built on open debate and commitment to a policy of always receiving the first blow before it strikes back. Whether they are answered, and in time, will depend upon the leadership and responsibility of the Federal Government.

Realistic defense measures for the national economy require an entirely different set of guiding principles than those used in past planning. Government, at all levels, must strive (i) to reduce the effects of enemy attack on people, cities, industry, and government; (ii) to assure continuity of basic governmental functions, essential production, and vital community services; and (iii) to deal with emergency conditions resulting from attack and restore essential facilities necessary to support the national security while maintaining the will to fight.

A fundamental principle in nonmilitary defense preparations, and basic to the afore-mentioned governmental objectives, is that of providing the earliest possible warning of attack. In the public domain is the recent authorization of seaward and landward radar extensions to assure early information of mass attacks on this country. This is a strictly military phase of early warning, made effective by military discipline, military weapons and manpower, and a military evaluation of the threat.

At the other end of the communication line are some 160 million citizens whom the military is sworn to protect and upon whom the military is dependent. It is to the concern of all that a military decision regarding attack imminence be passed to those citizens at the earliest possible moment and in a form that they can translate easily into the action that time permits. Time has become the keystone to individual and national survival. At the Federal level, action is being accelerated to assure maximum possible warning to all citizens. Irrespective of the type of warning, citizens must know what it is, realize what it means, and be prepared to act. This, in the final essence, is not an organizational but an individual problem of everyone. It is a vital part of "total preparation."

Equally basic to nonmilitary defense preparation is the principle of dispersion. It is axiomatic that, as the destructive potential of a single weapon grows, the damage potential of that weapon must be reduced. The density of both population and productive machinery, therefore, must be thinned. Space, in application hereto, provides the best defense, since it does not become obsolete when newer and more powerful methods of attack are developed.

Through the tax amortization incentive of the Federal Government, the nation is making steady progress toward industrial dispersion. This does not mean an uprooting of established factories. Recommendations are that *new* industries or *new* plants locate at a safe distance from the boundary of a target zone, in order to obtain relative safety and still meet economic requirements. The extent to which transportation costs enter into these requirements may determine the extent to which regionally discriminating freight rates, still existent, should be analyzed in the interest of national security.

Local industrial dispersion committees, composed of business, municipal, labor, and civic leaders, are contributing greatly to this program by helping industrial management select dispersed, economically feasible sites for new plants. There are now 92 of these committees operating in most of the large metropolitan areas of the country. As a result, during the past year, 86 percent of the new plants costing $1 million or more, for which the Office of Defense Mobilization granted accelerated tax amortization, were located on dispersed sites.

Population density must be reduced to the point where individual cities are no longer worthy of attack. By minimizing the effectiveness of attacks, the vulnerability of big cities will be reduced. These cities are the source of much of the nation's strength and, likewise, potentially the greatest weakness. Acting under new legislation, the Housing and Home Finance Agency is initiating a comprehensive slum-clearance program aimed at reducing population density but, unlike previous programs, providing for the displaced population. The density of population in the centers of cities must be reduced, and part of this land must be converted to nontarget uses such as parks and free-

ways. It treats, however, with only one phase of the dispersal problem.

To insure movement of working personnel with the dispersed factories, forces, at the same time, factory relocations to sites so far removed that commuting becomes undesirable on a permanent basis. An accompanying campaign by each company is required, therefore, to sell its employees on the desirability of moving. When full information regarding community facilities and housing conditions is furnished to employees, companies can retain a large percentage of their trained staff. They will retain even larger percentages as they work with home-financing agencies and the banks to extend metropolitan home-building interest rates to smaller communities.

A third basic principle of defense is that organization, procedures, and working relationships required for successful war administration must be in operating readiness in advance of an attack on this country. An orderly progression of international crises until a declaration of war is obtained, as in the past, cannot be assumed. A peacetime government must be prepared to convert in a matter of a few hours to one organized to meet wartime responsibilities. It is known that the country could not survive for long if essential governmental functions were destroyed or long disrupted. Those functions, therefore, have been identified—and the attendant numbers of people, equipment, and space needs, together with communications and other requirements—on a minimum basis.

The Office of Defense Mobilization has been exploring the means of locating governmental functions where they cannot be disrupted by enemy attack. There is no intention of abandoning Washington as the seat of the Federal Government. Instead, emergency relocation centers for essential functions have been selected in an arc from 30 to 300 mi of the nation's capital. The plan is to effect this same kind of emergency relocation in all centers of Federal, regional, or area activities. Emergency relocation is, of course, only a partial step. To spread the target as widely as possible and thereby offer a potential enemy the least possible return for his efforts, dispersal should be on a permanent basis. The extent to which government at all levels demonstrates its ability to function under attack emergency may determine the extent to which the citizen *wills* to retaliate and keep fighting.

Essential functions must be identified for emergency relocation. Flexible plans are needed; nuclei organizations must be extant, ready to swing into full mobilization action. These requirements call for approved organization plans, staffing patterns, and procedural instructions. More than this, they call for operating assignments to be given to personnel now performing nonessential work and for training of Federal personnel and executive reserves in their wartime essential functions.

Finally, plans and administrative mechanisms must be geared to reality by "war-gaming" these plans against a variety of attack assumptions. The operating readiness of the nuclei wartime organizations designed to meet those assumptions must be tested for near perfection. In this connection the Federal Government must take immediate steps toward the decentralization of decisions to responsible heads of regional offices and major field establishments. Until realistic delegations of emergency program and day-to-day administrative authority are made to these officials, it will be impossible for the Federal Government to meet the kind of administrative crises that would result from a sudden attack. No Federal coordinating mechanisms now exist in the Federal administrative areas, and present differences in authority and regional boundary result in an administrative patchwork quilt. These matters do not apply to the immediate postattack responsibilities of the Federal Civil Defense Administration or the various state civil defense administrators. The very existence of desirably strong, legally created state civil defense administrations, however, introduces problems of Federal-state coordination in time of crisis, unless relationships are clearly worked out in advance.

Decentralized operations for defense and rehabilitation must be supplemented with strong central policy and program organizations. National program-determining and resource-allocating bodies must be operationally ready to evaluate early information of attack damage and issue policy directives to govern regional and local administrative groups. If they are not, emergency operations of local groups will expand past the rehabilitation stage and either (i) unbalanced requirements based on unreviewed local needs, (ii) regional competition, or (iii) telescoped demands on remaining storage and productive machinery may disrupt and impair the long-range programing necessary to maintenance of a successful military effort.

A fourth principle underlying nonmilitary defense preparations involves protective construction. The relation of such construction to protection from damage and, therefore, ability to recover industrial production rapidly is mentioned in a foregoing paragraph. It is axiomatic that recuperability of the productive machine is an essential element in national security.

Identification of general target areas through the Federal Civil Defense Administration and vital productive facilities through the interagency Industry Evaluation Board has been attempted. Positive plans to disperse essential plants, where possible, also have been initiated. Vital facilities that cannot be dispersed must be protected to improve their chances for survival. This means building or improving shelters in existing buildings or requiring structural protection in new buildings that must be located within the target zone. It means reducing fire hazards through better housekeeping.

States and cities should amend laws and ordinances to include standards of construction and zoning that will help improve ability to resist the effects of weapons. Architects, engineers, and builders are cooperating by including in their designs the measures necessary to help new buildings withstand the pressure of nuclear weapons. Further cooperation is sought from those who invest money in real estate. They must be urged to protect investments by locating them safely or building them to a higher standard of strength.

Priority of Federal efforts is placed on the plants that are most important to the defense effort and cannot be dispersed. An interagency Facilities Protection Board has been organized to pool the information in the hands of several agencies and to assist the Office of Defense Mobilization in developing standards for physical security that can be adapted by the management of individual plants to meet the specified situations.

As a final tenet of defense planning, the stockpiling of critical materials, machinery, and manpower to the maximum extent possible has been undertaken. There is the realization that the nation will have to fight the first 2 years of any new war on the basis of material and machines on hand and talent then available. There is constant reevaluation of assumptions and progress toward *materials* stockpile goals. Stockpiling of *manpower,* however, is a more fluid and complex problem, because manpower does not remain constant; it has a will of its own. Thus, there are in process an inventory of talents, a building and training of reserves of executive abilities, and a certification that technical training is available and is being stimulated in colleges and universities.

The Office of Defense Mobilization, in the Executive Office of the President, is responsible for planning and coordinating the mobilization of industrial and commercial might. It is its responsibility to see that wartime operating matters are clearly assigned so that duplications and gaps will be kept at a minimum. The ODM program areas are constantly examining transportation, production, communications, materials, manpower, and stabilization problems. And, as necessary, the responsibility for planning certain wartime operations has been delegated to existing Federal departments.

As with every new weapon, there are defenses against nuclear attack. They involve a combination of factors of early warning, dispersal arrangements, operational readiness, protection of vital facilities, and the stockpiling of materials and manpower. These defenses, like the weapon itself, involve expenditures of money, time, human effort, and even sacrifice, as never before required. With the country's existence at stake, every effort must be made to meet these terms.

Mobilization is no longer to be undertaken upon declaration of war or after war starts. Either we stay ready at all times to absorb and survive the worst blow an enemy can strike, or we do not. If we do not, there probably will be no chance to mobilize afterward. If we do, there is a good chance that the blow will never be attempted, for we will have substituted many targets for a few. The enemy will no longer be able to destroy effectively the capacity to operate and, thus, to break the national spirit by a massive attack on major metropolitan areas.

Recent Developments in Civil Defense Policy

Under the Federal Civil Defense Act of 1950, civil defense is designated as the responsibility primarily of the states and their political subdivisions. The structure of civil defense is thereby consistent with the American philosophy of government. Beyond that, it becomes the only workable way in which to approach the problem. The only forces under Federal control, even under attack, are the employees of the Federal Government other than the military. The only forces a governor controls are generally the mobile support groups that he is authorized by statute to organize directly to supplement the civil defense of the subdivisions. This decentralization makes it possible for some states to subscribe to some measures and theories, some to others, and, unfortunately, some to none at all. The task of the Federal Civil Defense Administration, therefore, is essentially one of research and consultation.

Civil defense consists of two broad phases. The first phase immediately precedes the explosion of atomic or hydrogen weapons. The second phase is the phase following such explosions. Basic practices and procedures followed in the second phase are largely those devised by the British in World War II. Here, reference is made to such things as the development of auxiliary police forces to main-

tain law and order, auxiliary fire departments to fight fires, the organization of transportation facilities of the community and of communications facilities of the area, the development of block wardens, and the organization of medical and hospital facilities of the region involved. Since World War II, many things of a protective nature have been and are being learned from the German experience. Techniques employed in the Scandinavian countries are studied, all three of which have excellent civil defense organizations. Other nations, as well, are developing civil defense—Canada, Turkey, Portugal, Italy, and certain other areas or countries of the world.

The Civil Defense Administration has been striving in recent months to develop new techniques for the first phase, the phase immediately before atomic weapons are exploded. In this phase civil defense has just one tool—utilization of space. In February 1953, the Civil Defense Administration, although aware of what had happened in Eniwetok in November 1952, was still planning in terms of the explosion of atomic weapons the equivalent of 50,000 tons of TNT. The weapons that were exploded over Japan were the equivalent of 20,000 tons of TNT in explosive force. The documentary records of Operation Ivy, however, required a reevaluation of some major tenets of civil defense policy. Explosive force now was that of a weapon the equivalent of several million tons of TNT. Operation Ivy opened a hole in the island of Elugelab 175 ft deep and created a crater a mile wide, literally blowing away the entire coral island. Since that time, in March 1954, another explosion in the Pacific has occurred of a weapon that was much larger than the one that was portrayed to the nation in the film release "Operation Ivy."

Today, in the Federal Civil Defense Administration, planning is in terms of the possible utilization in a third world war of weapons of 10 and 15 million tons of TNT explosive equivalent. Actually, the end of explosive force is not in sight because there seems to be no reason why weapons could not be made the equivalent of 25 million tons or 50 million tons or more of TNT. This means that if such a weapon is dropped, for example, over the San Francisco Bay area, the area would be obliterated. Or, likewise, over Los Angeles, a large portion of the city would be pulverized.

In view of these facts, in June 1953, suggestions first were made that the only hope to save the lives of the American people in the event of an atomic war was simply to evacuate target cities. Preattack evacuation has emerged, thus, as the best hope of survival.

The question might be asked whether or not cities can be evacuated. A number of exercises have been effected and more are to follow. A careful study of the question has been made by reputable organizations competent in the field. So far developments indicate that evacuation is possible. It can be accomplished.

The first test conducted was in Spokane, Washington, a city of 160,000 population. In the downtown area, 16,000 people were moved out of the office and industrial buildings on foot, a distance of some 7 to 8 blocks, in the rain, in 8½ min. In Mobile, Alabama more than 30,000 people were moved by automobiles and public transportation from the downtown portion—leaving only guards—to the outskirts of the city in 22 min. In Houston, Texas, which is now a city of more than 600,000 population, a preliminary test moved all the automobiles out of the downtown area, an area of more than 400 blocks, within 6 min. Albany, New York, and other cities, have had trial tests.

In Milwaukee, Wisconsin, the Wilbur Smith Associates of New Haven, Connecticut, a traffic engineering firm, and the Traffic Control Institute of Northwestern University were employed to make a study of the sufficiency of the roads and highways leading in and out of Milwaukee and of the traffic control procedures that would be necessary to determine whether it would be feasible to evacuate 800,000 people from the city of Milwaukee. The conclusions estimated that four-fifths of the metropolitan area population could be evacuated from probable bombing range, north and west, within 3 to 7 hr. The report indicates further that, by widening some roads at the outskirts of Milwaukee, evacuation can be made even more feasible. It is believed that speculation no longer exists concerning the problem; given adequate warning time, people can be evacuated. Even though evacuation might not be 100-percent successful, any evacuation is better than no evacuation. The Smith study is being converted into a basic traffic pattern for the planning use of all cities.

The foremost study in civil defense, to the knowledge of the author, was made by the Associated Universities of the East. The study is known as Project East River. In Project East River it is stated that the only way civil defense can be made to operate is to make it manageable, and to make it manageable three things were required: (i) dispersal of industry and population, (ii) adequate warning time, and (iii) effective air defense.

Progress in the areas of early warning systems and effective air defense is being attained at a rapid rate. The dispersal of industry and population, however, is not proceeding at a satisfactory rate.

This aspect of civil defense must be stimulated. Dispersal, in the final analysis, is the most important tool of passive defense. It has some implications that are not pleasant to those who own property in the center of a city, just as the atomic bomb may not be pleasant to those who own such property or who may be in the center of the city. But it is necessary to disperse industry and population. The more hopeful aspect of the matter is that a normal trend toward dispersal is now being effected in the United States. Dispersal is proceeding, if at only a normal rate, from the centers of large cities to outlying areas.

A major problem of civil defense is complete evacuation. New York, for example, presents, perhaps, the worst evacuation problem in the world. The principle here, of course, is that a thinning of the population of cities means that lives will be saved. Two kinds of evacuation are necessary. There must be what is generally called strategic evacuation. Such would be undertaken any time the world situation worsens to the point that it becomes clear that war is imminent. The aged, the infirm, and the children should be removed from the large cities, as they were in England during the last war. Second is tactical evacuation—which starts when information indicates that enemy bombers are approaching. In this regard, estimates can be made that from 3 to 4 to possibly 6 hr of warning time can be given, depending upon the location of various communities within the landmass of the United States.

There are three effects of an explosion of an atomic weapon: (i) a blast effect which pulverizes, fractionalizes, and vaporizes everything at the point of the explosion; (ii) a fire effect, covering the number of miles that the flash covers, dependent upon the atmospheric condition on the day that it occurs and upon the size of the weapon; (iii) the radioactivity that is created by the explosion of the weapon. Unfortunately, there has been a tendency in the United States to minimize the effects of radioactivity. It appeared for a long time that atomic weapons would probably be detonated several thousand feet in the air, since, by detonation at that height, the enemy would be able to obtain the maximum amount of blast effect over the widest possible area. Of course, if a weapon is detonated 2000 or 3000 or 4000 ft in the air, radioactivity is not created to the same extent that it is if detonation is made into or close to the ground. However, new investigations in this field indicate, that with the development of these tremendous weapons in the millions of tons, an enemy could drop a hydrogen bomb on, for example, the San Francisco Bay area and get all the blast effect desired or needed and all of the fire effect needed by detonating the weapon either into the ground or close to the ground and, in addition, could throw in the air tremendous quantities of pulverized, radioactive material. The heavier particles would fall close to the explosion and the lighter particles drift for distances in the higher altitude. When a bomb first explodes there is a puffout or a throw-out of radioactive material, which extends only a few miles, again dependent upon the size of the weapon. Then the surface winds move some of this material. But surface winds are of less concern than are the higher winds. The winds from 10,000 to 50,000 ft high appear to cause the greatest problem. These winds ordinarily move around the earth from a westerly direction, varying from summer to winter, from southwest to northwest. Studies are going forward in this field, and useful information is being gathered.

The civil defense problem is complicated, therefore, not only by the blast and the fire, but also by the problem of radioactive material. Obviously, evacuation must not be downwind. Evacuation must be away from the path of this material as it goes out over the countryside. Eventually information concerning the fallout pattern must be made available in the most complete manner to the American people, since it will be their lives that will be at stake. The matter is applicable to rural areas as well as to city populations.

It is fortunate that radioactive material decays rather rapidly. Most of it would decay in a matter of hours. Some of it, of course, would not decay for a long time. It would appear that anyone who can get into a large office building would immediately cut the hazard to some extent. If he could get into a basement, the hazard would be cut materially. But if he can get under about 3 ft of ground, it appears that there would be absolute protection against the effects of radioactivity. Possibly the best defense against radioactive fallout is to get into an old-fashioned Kansas or Nebraska cyclone cellar. If one could get inside a cyclone cave, with 3 ft of dirt overhead, with some kind of filtering substance in an air intake, safety would be assured. The length of such refuge would depend upon the indications of a Geiger counter. In no event would it appear necessary to remain within such protection more than 4 or 5 days. Everyone in the United States should have some kind of a backyard shelter thus indicated if residence is outside of the area of blast and fire. If residence is 15 mi from an assumed target area, it also would be advisable to have one of these caves for protection. It would guard against fire, it would offer some protection against blast, and it would

offer virtually absolute protection against radioactivity. It might be stated parenthetically that radioactive material can be bulldozed away, and it can be washed from buildings.

If the 172 major metropolitan target complexes in the United States were bombed, destruction would affect only about 3 percent of the land area of the United States. Thus, there still would be places in which to fight, still reasons for which to fight. This does not minimize, however, what would happen in this country if an atomic bombing occurred of the kind a prospective enemy is now capable. If an enemy caught the people of the United States in its cities, there would be millions of dead immediately, and more millions of people injured. There would not be enough doctors, nurses, veterinarians, dentists, and first-aid people to take care of the injured.

The United States was not planned with atomic warfare in mind. Only one city in the world is planned for the atomic age—Darmstadt, Germany. It was bombed by the English one night in World War II. There was a strong wind spreading fires so rapidly that fire engines were ineffective and thousands burned to death. And now the Germans have rebuilt Darmstadt. Fire alleys were constructed—roads so wide they can never be obstructed. The Germans have required that if a person builds on his own private property fronting on those roads, he must build far enough back from the street line so that if the building collapses, it will collapse on the property and not on the public thoroughfare. Darmstadt is the only place in the world that has the type of streets that will permit evacuation in the event of an emergency.

If 20, 30, 40, or 50 American cities were bombed, what could be used for money after the attack? What would happen to the insurance companies of America? What would be used for food? In many cities the commercial foodstuffs are exactly in the target area. What could be used for transportation if the great harbors of America were bombed? In the New York–New Jersey area there are tremendous concentrations of petroleum, right in the target area. This condition exists throughout the country.

The Civil Defense Administration is working unceasingly on problems of medical, economic, and social nature. The mobile field hospital is a recent development. In transportation, the best system of emergency priorities is being established. Much work still must be done on matters of a political and legal nature. The care of documents and records requires attention by businesses and individuals. Continuity of courts and judicial administration must be provided. In most states the governor has the power to fill judicial vacancies. Although in many constitutions a clear line of succession is stated if vacancies occur in the governorship, in others it is silent.

Problems of estates and dependency matters also need attention. The majority of the great financial institutions are in the midst of metropolitan centers and are sure to be targets in an all-out attack. Measures are needed to allow for adequate credit, debt, and currency functions, for proof of deposits and use of negotiable instruments by evacuees. In a postattack period uneasy conditions would demand early maturity dates on loans, giving rise to moratorium problems. Techniques for clearing debtors of past obligations would be required. Questions of insurance risk arise. Corporations must have a succession of management. With essential civilian production reduced to a trickle problems of national morale would arise from unsatisfied demands for goods and services required to maintain life.

Within the past 3 years interstate compacts for emergency organization of critical target areas have been drawn. In most cases these call for voluntary relinquishment of some sovereign rights by political subdivisions. Some agreements have been reached between state and Federal governments voluntarily granting power to the FCDA regional administrators in the interest of common defense. The alternative to planning in these fields is national chaos or martial law. History has demonstrated, however, that martial law is neither essential nor desirable in civil defense operations.

The Civil Defense Administration has some of the answers to these problems; it is not certain that they are the right answers. American leadership in respective fields is invited to consult with and offer suggestions to the administration on general or specific matters. Leaders at all levels must recognize the importance of their actions in making civil defense effective.

There is no bomb, no collection of bombs, that can be exploded in this world as of this minute that can destroy the world. Whether such a holocaust will come sometime in the future is unknown. But today there is every reason in the world to be hopeful. Above everything, mankind must learn to develop the social consciousness and common sense to cease engaging in futile and foolish slaughter every 25 years or so and learn to live in peace.

Some Oceanographic Results of the Odyssey

W. E. MALONEY

The author has been a member of the Division of Oceanography, U.S. Navy Hydrographic Office, in Washington, D.C., since 1951. His undergraduate and graduate training in biology were taken at the George Washington University. Mr. Maloney is the coauthor of a forthcoming paper on tidal theory to appear in the Transactions of the American Geophysical Union.

THE plains of Troy which had rung with the sounds of battle for 10 years had long been still. The seduction of the fabulous city by a wooden horse had been accomplished, and probably even now men were building another city, one of six more to follow, atop the cold ashes. Priam, king of Troy, was dead, slain at the city's fall. The handsome Paris, lover of Helen, was dead, the victim of a poisoned arrow wound and the righteous wrath of his legal, river-nymph wife who refused to perform her healing arts on a faithless husband. Achilles, alas, was also dead; Priam's arrow, guided by the jealous Apollo, had found the fatal heel. Agamemnon, surviving 10 years of warfare, had returned to his home only to die at the hands of a guilty wife and paramour. All this had come to pass as the result of one of the greatest struggles of history and literature and to most of the principals, in one way or another, the end of this period was the end of their lives.

Not so, however, for the brave and crafty Odysseus who had incurred Poseidon's wrath for blinding the Cyclops and as a result was doomed to wander the length and breadth of the Mediterranean Sea at the whims of that god. For 10 years in the early part of the 11th century B.C., the hero of Homer's *Odyssey* was driven about the sea, experiencing adventures whose authenticity or symbolism are today still debated with interest and vigor.

Not the least of these experiences was the encounter with Scylla and Charybdis. As Homer describes it, Odysseus and his men must pass in their ship between two high cliffs. On the one side, the six-headed, twelve-armed monster Scylla lived in the cliff and was wont to gobble up six men from every ship that passed close by. On the other side, the whirlpool Charybdis thrice daily swallowed the water and spouted it back out. When she swallowed the water, she showed black sand beneath her whirlpool and the rocks boomed all around. When she spouted it out, the water swirled up in a seething mass and the spray rose high in the air. She could easily swallow a ship.

The oceanographer reading this passage from Homer is immediately struck by several facts: (i) the periodic nature of Charybdis' function; (ii) the obviously strong currents and countercurrents; (iii) the towering cliffs on each side that imply a narrow strait.

The Strait of Messina, the narrow channel that separates Sicily and Italy, stands out as the obvious setting. Indeed, to this day "Scilla Rock" is still to be found there. The tidal currents, coursing first one way then the other through the strait, are among the strongest to be found anywhere in the world. At times of spring tides, when the sun and moon combine their forces, the currents reach speeds greater than 5 knots at places within the strait. The lives of the seafaring people who live along the coast are so bound to the currents that specific names have been given to them. The north-going current is called the "Montante" while the

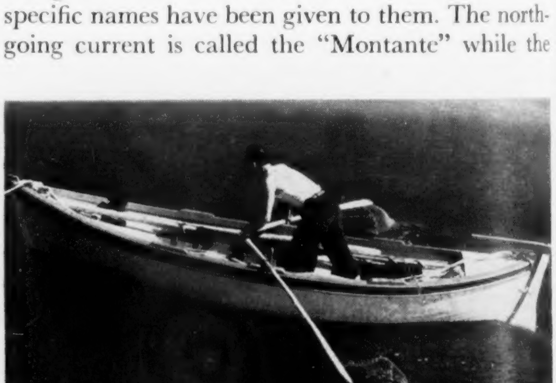


Fig. 1. A modern odyssey. Capt. Adrian V. Lane of the research vessel *Atlantis* shown rowing a Greek dinghy in the Mediterranean. [*Courtesy Woods Hole Oceanographic Institution.*]

southgoing is called the "Scendente." The currents change about every 6 hr and 12 min, thus having three reversals in a 24-hr period. Both the "Montante" and the "Scendente" cause particularly unpredictable countercurrents, called "Bastardi," to be formed close to shore.

How is it that we should find these conditions in the so-called "tideless" sea, the peaceful blue sea of sluggish currents? The answer is that long before the beauteous Helen ran off to Troy with Paris and the vengeful Greeks amassed before the city and long before quarrelsome man spread his puny civilization around the shore of the inland sea, the forces of nature conspired to form a unique set of circumstances.

The principals in this ever-continuous show are the moon, the sun, the two major subdivisions of the Mediterranean Sea known as the eastern and western basins, and the blue salty water that the basins contain. The Mediterranean Sea can be considered to be two large basins lying end to end, communicating through the fairly wide Strait of Sicily between Sicily and Tunisia and the narrow Strait of Messina between Sicily and Italy. The gravitational forces of the sun and moon passing over the sea from east to west cause the waters to "pile up" first at one end and then the other end of the two basins. You can illustrate this for yourself the next time you bathe. When the tub is about one-half full of water, place your hand in the water in the center of the tub and move it horizontally at varying speeds. Soon you will hit upon the proper rate of movement, and the water in your tub will begin to slosh back and forth as one body. Now you will see that when it is high tide at one end of your tub it is low tide at the other end, and vice versa. Now visualize two bathtubs placed end to end and connected by a narrow vertical opening. Suppose that the water in each tub is set in motion, as was previously done, but timed so that when it is high tide on one side of the opening it is low tide on the other side. It is apparent that the differing water levels that are being set up between the two tubs will drive the water through the opening first one way, then the other. This is analogous to the situation between the eastern and western basins of the Mediterranean. When it is high tide at the southern end of Messina Strait, it is low tide at the northern end and the "Montante" rushes northward. About 6 hr and 12 min later it is high tide at the northern end of the strait and low tide at the southern end and the "Scendente" rushes southward. Even though the rise and fall of the tide in the Mediterranean Sea is slight, the hydrostatic force set up by the differing tide levels of the water in the two basins causes these rapid currents in the narrow strait.

Fig. 2. Three thousand years after the wanderings of Odysseus the research vessel *Atlantis* of the Woods Hole Oceanographic Institution made a 6-mo odyssey in the Mediterranean to obtain ocean-current measurements, to chart and photograph the bottom, and to investigate the productivity of that sea. [*Courtesy Woods Hole Oceanographic Institution.*]

It was the goddess Circe who tipped off Odysseus concerning the dangers he would encounter between "Scylla" and "Charybdis." Regardless of her beauty and divinity, however, we must relegate her to the position of a mere descriptive oceanographer, for, had she truly understood the things of which she spoke, her advice to Odysseus would have been, "Set thy sail, most admirable Ulysses [Odysseus], at a time such that you may pass between Scylla and Charybdis when the ocean waters stand half high and half low, for then Scylla sleeps deep within her cave and Charybdis neither sucks in nor spouts out the water."

Wanted: More Ivory Towers

WARD PIGMAN

Dr. Pigman is associate professor of biochemistry at the University of Alabama Medical Center, Birmingham. Before going there he served with the National Bureau of Standards, the Corn Products Refining Company, and the Institute of Paper Chemistry. He received his training at George Washington University and the University of Maryland. In 1938–40 he was Lalor fellow at the University of Leipzig. Dr. Pigman, whose chief field of research and interest is the carbohydrates, is an editor and founder of Advances in Carbohydrate Chemistry.

WITHIN our recent memory scientific research has changed from the status of an avocation of scholarly individuals to that of a major social force. The public now anxiously awaits the announcement of cures for cancer and polio; there is fear that the Communists may discover a still bigger H- or L-bomb; and published reports of new drugs or of the possible influence of smoking on the incidence of cancer affect the stock market the next day.

In the development of research, Birmingham is representative of the general expansion that has occurred in the South. Ten years ago very little research was being carried out; since then former slum areas have been replaced by modern research organizations, including the University of Alabama Medical Center, the Southern Research Institute, and several industrial laboratories. All over the nation, a similar resurgence has occurred. National meetings formerly attended by hundreds are now attended by thousands. According to the National Science Foundation, a 20-fold increase in funds spent by the Federal Government for research and development took place between 1940 and 1954; $97 million were spent in 1940, and $2200 million in 1954. These sums represented 1 percent of the federal budget in 1940–43 and 3 percent of the much larger total budget in 1952–54.

The appearance of science as a major social force has created many problems. We are justly proud of our American civilization. But how much scientific culture does Mr. Average American have? Our educational system has failed miserably in raising the culture of our people to an appreciation of the need of basic science. There is even less of what James B. Conant has called an "Understanding of Science."

Mr. Average American has not been taught to think critically or to weigh evidence. In the current controversy over the incorporation of fluorides in drinking water the public uses personal testimonials on the same basis as scientific evidence. Should our amusement not be tempered with sadness when we read "I found that drinking one bottle of Hadacol cured my rheumatism?" With even more sadness, we find some scientifically trained people thinking in the same fashion. In the battery additive hearings involving the National Bureau of Standards and Pioneers, Inc., the issue was whether material AD-X2 (a mixture of sodium and magnesium sulfates) would, when added to a battery, prolong its life. Several technically trained individuals said they were sure it would work because they had tried it!—they spoke in the same way as the Hadacol users at whom they would laugh. An Assistant Secretary of Commerce used the same argument—a personal testimonial. The director of the National Bureau of Standards almost lost his position over this issue.

The persons involved in these two examples were not all people with a limited education; some had had an above-average education, and some even a technical education. In very few areas of our educational system have the basic facts and methods of science been properly incorporated and taught. The result is that the majority of our people are scientifically "illiterate." This creates an extremely dangerous situation, anticipated by Goethe when he said, "There is nothing more frightful than an active ignorance."

The patent laws provide another example. Our highest court not so long ago, developed the principle of "the flash of genius." An invention could not be made by a systematic planned attack. An invention was like a stroke of lightning from an unclouded sky. It had no apparent origin or reason. If it did, it was not an invention.

Discoveries take place when a definite stage of scientific, technologic, or clinical development occurs. They are then almost inevitable if the facts are known to persons facing specific problems. The outstanding example of this was Thomas Edison, whose genius led to many practical discoveries. Without the basic studies of electricity during the

preceding century, many of Edison's discoveries would not have been possible.

The weaknesses in the patent law have been realized, and in the Patent Act of 1952 the earlier requirements were relaxed. It may be predicted, however, that additional modifications will be needed in the future; the legal qualifications of an invention must be in harmony with the real manner in which discoveries and inventions are made. The ignorance of the public in this respect can be illustrated by a request once made by the manager of a radio station that a discovery be made some morning and recorded on tape so that he could broadcast it later at his convenience!

The same ignorance of science is apparent in some of the current controversies about secrecy in scientific matters. Scientists, for example, Oppenheimer, are even accused of disloyalty because they have defended free technical discussions and open publication. Educated, intelligent, but scientifically ignorant people do not know that the whole future development of science is based on open exchange of information and free criticism. They do not know that the basic secret of the atomic bomb was published in scientific papers and known to the community of scientists before its application was obvious.

Excessive secrecy will destroy all scientific progress and will be as effective for this purpose as bombs placed under each of our research laboratories. Because of the very nature of science it is either impossible or at least highly inefficient to have an American science or German science or a Russian science. Certainly there are no special American or Russian atoms or French molecules. Goethe said, "Science and art belong to the whole world, and before them vanish the barriers of nationality." Our scientists are creating a united world, while our politicians are endeavoring to keep it split. Which is the way to peace, happiness, and prosperity?

These fruits of the tree of scientific ignorance are so rotten that a stench is arising. In other situations the difficulties are more subtle but essentially just as important. One of these, although it is somewhat abstract, is a problem that will profoundly affect our future comfort, health, and national defense. It is partially a fruit of ignorance and partially one of semantic difficulties. It is the meaning of the term basic research.

The term research is itself a source of confusion. Research means simply a careful systematic or diligent search for facts or principles. It comes from an old French expression meaning "to seek again." In general and proper usage, one finds library research, historical research, legal research, industrial research, clinical research, and scientific research. *Scientific* is no more a necessary adjective for *research* than *damn* is for Yankee! Scientific and basic research are special restrictions of the general term.

Basic research is simply our effort to understand the elementary composition and operating principles of the world in which we live. It ultimately leads to the development of concepts such as electrons, atoms, molecules, chromosomes, cells, energy, electricity, radio waves, and to generalizations such as the Mendelian laws and the laws of thermodynamics. The reason for society's support of research is that with knowledge of causes comes power to control the results. Claude Bernard, the father of modern physiology, described the object of experimental science as "to discover the laws of natural phenomena, for the purpose not of foreseeing them but of regulating them at pleasure and mastering them."

Basic research frequently is highly specialized. Because of its low state of scientific culture the public cannot even understand the nature of some of the problems. How is it possible to explain the nature of an abstract problem in physics or chemistry? One would have almost insuperable difficulty in explaining in simple words studies of inductive effects in organic reactions, Walden inversions, entropy changes in enzyme reactions, and organic syntheses. Even scientists in one field are unable to understand the problems of other fields. The late C. S. Hudson once said slightly facetiously but with much wisdom, "Basic research is research in which the only person with much interest in it is the investigator himself."

Basic research usually and perhaps always has no apparent purpose. This quality particularly disturbs the nonscientist who cannot understand the reasons for the public financial support of work that will lead to no known immediate result. Perhaps the situation can be made clear by likening basic research to the explorations of the 15th and 16th centuries during which the physical geography of the earth was made known to Europeans. Society financed these explorations without knowing specifically what could be found. Columbus, financed by the jewels of Queen Isabella, sought the Indies and found America instead. Our modern explorer—the basic scientist—can no more really predict the nature of his discoveries than could Columbus. He must explore before reporting his discoveries.

When the basic knowledge of our universe has reached a definite stage of development, persons with problems in specific fields may find in the prior basic discoveries the solution to their specific

problems. This application is what is known as a practical discovery, an invention, a cure, or an industrial process. The value of iron could not have been predicted before its discovery. The basic studies of electricity and radio waves were the bases of electric motors, radios, and television, and of our modern industry and manner of living. Modern chemotherapy has been a side result of the past century's elucidation of organic chemistry. Who can now foretell what wondrous properties and materials of our universe still remain to be discovered?

The process of adapting basic discoveries to the use of man is called variously clinical research (in the medical and dental fields) and developmental or applied research (in the industrial field). Such research may be readily understood by Mr. Average American. Nearly always, a direct purpose or problem is involved. The problem may be the best way to cure a sick person, or the least expensive way to make a useful product. This type of research has high importance as the last stage in the utilization of information created by basic research. It requires much ingenuity and skill, usually with very little actual knowledge of basic science, but with an understanding of its results. The essential difficulty may turn out to be the finding of a suitable solvent or of a proper piece of equipment.

Another type of research is common, but its special existence is not generally recognized. In current research, for example, numerous workers have reported that it is possible to produce decay in extracted teeth by placing them in a nutrient solution that has been inoculated with bacteria isolated from a human mouth. With the purpose of demonstrating how tooth decay occurs, an apparatus called the "artificial mouth" was developed. The procedure is to take sound extracted teeth and expose them to bacteria from the mouth. To keep the bacteria alive, a solution containing food needed by the bacteria is allowed to drip constantly over the teeth. This simulates the flow of saliva over teeth in the human mouth. It was found that teeth could be completely destroyed by this treatment. Since natural decay is usually highly localized, especially in pits, the artifical attack seemingly was too general. However, the decay could be restricted to regions much like those naturally seen if the teeth were brushed at least twice daily. This seemed to be a result of the removal of bacteria from the cleansed surfaces. In the human mouth this is probably accomplished mainly by the rubbing of the cheeks and tongue over the tooth surfaces.

Later it was found that the presence of certain sugars in the liquid running over the teeth is very important. Sound teeth remain unchanged for long periods, if the amount of sugar is small (<0.10 percent). With 0.5 percent glucose in the liquid, the enamel of the tooth was attacked; the dentin appeared sound but had lost its mineral matter. When about 0.2 to 0.3 percent glucose was present, the entire tooth was attacked and destroyed. Apparently the decay process can be markedly influenced by the amount of some sugars in the saliva, a fact long recognized by dental research workers.

Clinicians might call the work with the artifical mouth basic research. Basic scientists might call it clinical research. Probably it is neither. Several years ago, the late C. M. A. Stine, vice president of the DuPont Company, pointed out that a distinction should be made between basic research and what he called "pioneer industrial research." By this he meant research in which the basic variables of a process are studied. However these variables are not directly explainable in the terms of the basic sciences. Edison carried out much such research. For example he investigated the possibilities of many different materials as filaments for his electric light bulb. His carbon filament worked, but there was no known reason for its success. The chemist and pharmacologist operate similarly when new drugs are made and tested empirically.

Essentially this type of research may create an isolated body of scientific knowledge. It creates a special science, not integrated with the principal body of basic science and not interpretable on the basis of established previous scientific principles and concepts. Usually a fairly definite purpose dominates the work, and the results will have a directly useful application or significance.

Such research might be called pioneer or Edisonian research to distinguish it from basic research or from applied industrial or clinical research. Pioneer or Edisonian research has much value but should not be confused with basic exploratory research and particularly should not supplant it.

Much confusion can be avoided if these distinctions in types of research can be kept in mind. All are necessary. All require intelligence and ingenuity. But the skills and training needed may be quite different. Their combination is necessary to make new discoveries useful to society. Yet it must be kept in mind that the basic research carried out over the past several centuries has been the seed from which the current improvements in health and living have blossomed.

Our scientists are our modern explorers, push-

ing back the frontiers of the "land of knowledge." This is a land not on this earth or in the heavens above, but composed of them and of the minds of men. It is a limitless land fraught with difficulties for the explorers. Towering mountains, rushing torrents, arid deserts, pestilent swamps, and tangled jungles must be penetrated and conquered. Unforseeable riches in well-being and material comfort are to be found. Perhaps even the "fountain of youth."

For centuries the mind of man has been exploring this land. A small portion of it has been settled. Here the original explorations have been utilized for the benefit of mankind. Around the densely settled region the explorations have been progressively less extensive. Beyond the frontier are rich, boundless unexplored areas, the exploration of which will return great dividends for a society courageous enough to support the explorers.

As the border regions are explored and become a part of the "land of knowledge," the frontiers move outwards. The explorers are followed by settlers and industrialists who exploit the original discoveries for the benefit of mankind. Finally the areas become fully populated, the results of the original discoveries are fully utilized, and progress only can continue by an expansion of the borders into new unexplored areas. Progress ceases unless society continues to support the basic explorations on the frontiers.

Our basic science laboratories are the modern version of the frontier forts or of the towers on the borders of the Roman Empire. But unlike the early towers, these are towers of ivory, places of work and shelter not readily defensible against physical assaults.

The poetic concept of the ivory tower is a place of repose and meditation. Wilfred Rowland Childe wrote:

> A tower of ivory it is
> Beside a shoreless sea;
> I look out of my lattices
> And the saints appear to me.

Incidentally, the first known use of the ivory tower in this sense was apparently by the French poet Sainte-Beuve who in 1837 compared the contemporary writers Victor Hugo and Alfred de Vigny in the following words.

> Hugo, strong partisan
> . . . fought in armor,
> And held high his banner in the midst of the tumult:
> He still holds it; and Vigny, more discreet,
> As if in his tower of ivory, retired before noon.

Apparently there must have been still earlier use of the term.

Time for meditation, protection from distractions, and provision for the daily bread are an important prerequisite for scientific research. Charles Darwin in *The Origin of Species* said:

> The presence of a body of well-instructed men, who have not to labor for their daily bread, is important to a degree which cannot be overestimated: as all high intellectual work is carried on by them, and on such work material progress of all kinds mainly depends.

Our scientific ivory towers are not God-given. They must be constructed and maintained at considerable public expense. In providing for their construction and maintenance, society is making diversified investments with the dividends to be paid in the future. Although one investment may not be successful, others will more than make up for the poor ones. Our past scientific and technologic progress provides ample proof of the wisdom of these investments in basic research.

Our exploration and conquest of the "land of knowledge" can be expedited if society constructs an increasing number of ivory towers. Construction will not suffice alone. Our towers need to be guarded by staunch administrators, whose aim is not personal advantage or power but unselfishly to expedite the explorations by providing the necessities for work and life. Our explorer scientists have a heavy responsibility under this arrangement, and individual failure will bring criticism on all and retard the advance of science.

These ventures require the full cooperation of society, administrators, and basic and applied scientists. Mutual respect for the part each is playing is essential. We must take the same chances taken by Queen Isabella in financing Columbus' voyage. Some of our modern explorers will return, not with the riches of the Indies, but with much greater riches in eventually improved health and living conditions.

Curiosity is one of the most permanent and certain characteristics of a vigorous intellect.—Samuel Johnson.

Organic Detritus in the Metabolism of the Sea

DENIS L. FOX

Having moved to this country from his native England in 1905, Dr. Fox received his training at the University of California and Stanford University. From 1925 to 1929 he was a chemist with the Standard Oil Company of California; since 1931 he has been at the Scripps Institution of Oceanography, where he is now professor of marine biochemistry. In 1938–39 Dr. Fox was a Rockefeller Research fellow, and in 1945–46 a Guggenheim fellow. He is an occasional consulting biochemist and the author of Animal Biochromes and Structural Colors.

THE earth's hydrosphere, comprised largely of the oceans, is a heterogeneous system of particulate substances, ranging in dimensions from visible aggregates down to colloidal micelles of solid, liquid, and gaseous character, dispersed in an aqueous saline solution. The salt concentration, moreover, shows wide variation, for example, from a few fractional parts per million in mountain lakes and streams, through increased amounts in estuaries and river mouths, reaching values of 3.3 to 3.5 percent in the sea, and values nearly 10 times as high, or full saturation, in land locked salt waters.

The distribution and concentration of leptopelic (*1*) or finely suspended organic and inorganic materials in natural waters likewise show wide variation; minimal quantities are present in clear alpine lakes, while the highest concentrations prevail in stagnant tropical or subtropical swamp waters. Intermediate but greatly varying amounts are encountered in the oceans. The relative populations of microplanktonic organisms and the corresponding concentrations of their particulate detritus are partly responsible for the various green, yellow-green, ruddy or brownish colors of near-shore areas, while the blue-green or blue shades of deep oceanic areas represent the clearest waters.

The relative quantities of organic (for example, protein, lipid, polysaccharide, and humus) and inorganic leptopel (insoluble silicates, phosphates, carbonates, hydrated oxides, and so forth) and the ratios between the two kinds of suspended materials exhibit high variability even in temperate regions; the kinds, concentrations, and relative proportions between these two major classes depend upon temperature, relative density, water movement, season, light, and distance from shore (*2*).

Concentrations of organic leptopel in marine waters may vary from a few fractional parts per million in midocean or at great depths, through values of several milligrams per liter near the surface of near-shore areas, and up to tenfold or more in surface slicks or just beneath them. Quantities of particulate organic matter, adsorbed as pelogloea to solid surfaces, may exceed the amounts suspended in water by 10 times in beach sand, which commonly involves about 1 percent organic matter or by 100,000 times in fine silt or bottom muds (*1*).

The ocean might be regarded as a vast, loosely integrated but complex and actively metabolizing "cell," its somewhat salty but organically dilute "sap" gaining some of its dissolved nitrogenous and phosphatic nutrients from land drainage, some directly from electric storms and washing of the air by rain, and a considerable fraction through bacterial agents in the marine biochemical cycle. The ocean's fluid medium executes many well-defined currents, thus implementing a vast, complex circulatory system (*3*). Thus nutrients, dissolved gases and solid or semisolid inclusions are transported, while fluid masses of differing original salinities and temperatures receive partial mixing. A "respiratory" exchange of CO_2 and O_2 takes place between the ocean's surface and the atmosphere, and an immense "transpirational" cycle of water likewise occurs through evaporation and reprecipitation as rain. Photosynthesis is carried out on an enormous scale by green phytoplankton and by sea weeds of the euphotic zone. The circulating watery fluid bathes innumerable organized tissues, maintaining their osmotic integrity, bearing the finely divided food particles for their direct or ultimate somatic maintenance, while providing for the dispersal of their reproductive products and their cata-

bolic wastes. Certain solid, more chemically inert wastes may be gradually "excreted" from the central mass, finding their way to the ocean floor.

High variability within the oceanic cell applies not only to the organic content of its waters but to local differences in the intensity of metabolism. Minimal levels of total organic carbon and organic nitrogen may prevail, for example, both at warm equatorial latitudes and in deep, cold waters of the central Pacific, while the total biochemical turnover may reach its peak in temperate latitudes at and near the land mass. There are great seasonal variations in the metabolism of the sea in polar regions, biochemical processes reaching their low ebb in the winter freeze, and achieving a maximum with the spring thaw.

This article (*4*) is primarily concerned with the distribution, measurement, and metabolism of organic matter in its suspended (leptopelic) state and in its adsorbed (pelogloeal) condition. Organic matter in true solution is commonly but a minor fraction of the total (*5*). In practice, the colloidal or other particulate material is collected on microfiltrational membranes of cellulose acetate and nitrate, whose porosity retains bodies as small as 0.5 μ, or preferably 0.2 μ, in diameter. Staining, washing, and drying the filtered residue *in situ* are followed by homogenizing the membrane to light by the addition of an inert fluid of equal refractive index. Numbers, size-distribution, and total volume and weight of cellular material may then be estimated microscopically. Ashing of sufficient material so collected will yield values of inorganic leptopel concentration (*2*).

Organic leptopel is determined chemically by passing the water through a layer of fine, inert inorganic powder, for example, MgO and refined celite, capable of adsorbing the finest colloidal micelles, even such as molecular hemoglobin with a mean diameter of only 5 mμ (*5*). Complete oxidation of the collected sample will yield measurable CO_2 and NH_3, or will leave a titratable quantity of standard oxidant, such as chromic acid. Alternatively, total ether-soluble lipids or total diagnostic pigments such as carotenoids or porphyrins may be recovered and measured.

The common progenitor of the first living, reproducing biochemical complexes involved the hydrosphere, a vast heterogeneous "cell" bearing a great diversity of organic and inorganic compounds from which the newly formed aggregates were able selectively to assimilate their nutritional supplies (*6*). Unorganized organic matter greatly preponderated over the living kind and still does, but, as organisms have evolved, great changes have occurred in the kinds of nonliving organic compounds of the environment. It is profoundly questionable whether living matter is still arising *de novo* in today's world, for there is now a plentiful supply of atmospheric and dissolved oxygen, as well as a ubiquitous population of microflora, both of which agencies would promote the early destruction of any new, labile molecules (*6, 7*). The lifeless organic matter of the environment is made up largely, if not totally, of biochemical detritus, that is, from catabolic wastes, undigested residues and parts of dead organisms.

Large and diversified populations of organisms will be supported in regions of high content of unorganized matter. The two so-called "states" of organic matter exist inseparably in a dynamic state of flux, barring ephemeral dislocations from outside influences. Still, in any relatively equilibrated system of this kind, or in the marine world taken as a whole, the mass of unorganized detrital organic material must exceed considerably that of the collective living entities. Such a condition is a cumulative heritage from the primary photosynthesizers and is indispensable for the continuance of life as we know it.

Since true organic solutes can hardly exceed a small fractional part per million in most marine waters (*5*), they are inaccessible directly to animals and are doubtless consumable only very slowly by bacteria (*8, 9*). But organic matter in a dispersed, suspended condition, whether free or adsorbed to inorganic suspensoids, is a readily utilized source of food, since filtering or suspension-feeding animals are able to resolve and consume the disperse phase of a fine "puree," while they cannot utilize a thin tea or "consommé" of true solutes.

Let us survey briefly the metabolic activities of three representative marine animal species that feed upon suspended or adsorbed detrital matter. The sea mussel, *Mytilus californianus*, is a typical suspension-feeding bivalve; the polychaete beach worm, *Thoracophelia mucronata*, is a wholesale swallower of sand, and the intertidal snail, *Littorina planaxis*, exemplifies a large class of "scrapers" that cut away siltstone and its surface layer of pelogloea by means of a horny tongue or radula.

A population of 1 million mussels, for example, on an area of rocky surface, aggregating about ¾ acre, will, during their second year of life, filter the finely suspended material from at least 22 million tons of their environmental water, equivalent to a volume of ocean water about 1 mi square and 25 ft deep. If the suspended organic matter in this water amounts to about 5 ppm, the mollusks re-

move some 121 tons of such material. Augmenting their own collective weight of organic matter by 4.1 tons, they yield a ratio of filtered to incorporated organic matter of about 3.3 percent. By no means all of the filtered organic matter, however, is ingested by the mussel; much of it may be rejected by the gills and palps. Furthermore, a sizable portion of the organic material swallowed is not digested, for example, humus, chitin, cellulose, and other refractory materials, as well as numerous living dinoflagellates bearing impervious envelopes (*10–13*).

Manifestly these factors will combine toward a greater efficiency on the part of the consumer. An alternative but still conservative value for this efficiency may be derived from the data estimated by Fox and Coe (*14*) for a community of mussels, half of which are in their first year of life, about a third in their second year, and the remaining sixth in their third. A hundred mussels of such an age distribution will void an estimated total of 1960 g of feces in a year and will manufacture 210 g of gametes, while depositing a total of 139 g of new somatic tissue, all figures being on a calculated ash-free, dry-weight basis. These values yield a quotient of 17.6 percent efficiency. However, since the fecal weight, taken as the denominator, will be exceeded by the weight of material actually ingested, the figure for efficiency must be taken as only roughly approximate.

Benthic suspension-feeders, such as bivalves, tunicates, sponges, and the like, remove vast amounts of fine organic detrital matter from the sea, assimilating significant fractions of it as new tissue, while eliminating the greater portion in impacted fecal aggregates. Some of these aggregates settle to the floor of the sea, there enriching the nutritional supplies of mud-dwelling animals and the resident microflora. Freely swimming suspension-feeders, including many smaller crustaceans and salps, doubtless occupy an outstanding role in that portion of the marine biochemical cycle which involves organic leptopel.

The burrowing annelid worm, *Thoracophelia mucronata,* occupies damp sand in large numbers along the western North American coast, where it continuously passes sand through its alimentary tract, utilizing as food the relatively small quantities of colloidal and other finely divided pelogloeal material adsorbed from the water to the siliceous surfaces. A characteristic worm bed, 10 ft wide, may extend indefinitely along a sandy shore, limited chiefly by rocky barriers.

A worm bed 1 ft deep, aggregating 500 mi in length and 10 ft in width (that is, occupying an area of 1 mi^2), contains 2.64×10^7 ft^3 of damp sand, weighing 1.41×10^6 tons when dry. An average population of 3000 worms 1 ft^2 will give an aggregate of nearly 8×10^{10} animals in the bed, weighing a total 3500 tons. Since each individual may ingest about 2100-fold its own net weight annually, the community will be capable of cycling 7.3×10^6 tons of sand per year. Of the 7×10^4 tons of available organic matter associated with this quantity of sand, 700 tons will be converted into new tissue, assuming an annual life-cycle. This species thus demonstrates a value of about 1 percent for the ratio of ingested to newly added organic matter. A 2-year life-cycle would halve this value for the apparent efficiency. While not perhaps to be regarded as completely typical, this polychaete species may represent, in a general way, the metabolic pattern of sand- or mud-eating invertebrates (*15*).

We may consider, as a final example, a typical scraper or radulate feeder, the intertidal snail *Littorina planaxis* (*16*). This common periwinkle feeds on the small quantities of algae and pelogloea adhering to impacted Eocene and Cretaceous siltstone. It is a major agency in the erosion of the siltstone and in its return to the sea in finely comminuted form. Populations of the snail on tidally exposed siltstone near La Jolla, California, have been estimated to number about 333 animals per square meter. A snail with an 8-mm shell length (the predominant size) augments its tissue dry weight by about 7.2 mg annually. It ingests approximately 2336 mg of the siltstone per year. Since the consumed substrate involves about 2.6 percent organic matter, the ratio of newly incorporated to ingested organic matter is $(7.2 \times 100)/(2336 \times 0.026) = 11.9$ percent.

While metabolizing the organic material at this fairly efficient level, the calculated 8.6×10^8 snails on a square mile of wet siltstone will erode, in the period of a year, approximately 2200 tons, or 3×10^4 ft^3, of the siliceous material, fixing as tissue material, 6.82 tons of the total 57.2 tons of organic matter available.

The casual observer knows that the operation of this biological agency does not effect a homogeneous removal of a fractional millimeter per year from the surface of this stone, but instead results in the production of many roundish excavations of widely varying circumference and depth. These erosion basins occur not only in horizontal areas, where they remain filled with intertidal water, but appear in smaller sizes in sloping or even vertical surfaces. It has been estimated that a theoretical standing community of 100 *Littorina*

planaxis of average (8 mm) shell length, would be capable of excavating a liter of siltstone in 11.5 yr (*16*).

These random examples will perhaps provide some idea of the magnitude of only three diversified physical and chemical operations, continuing on a vast scale through the activities of countless marine invertebrate species whose nutrition depends upon ample supplies of suspended or adsorbed detrital organic matter.

Finally, in order to gain some concept of the mass of organic leptopel involved in the waters of the world oceans, let us adopt a relatively conservative value of 1 ppm as its average concentration (*1*). If the density of this organic detritus is approximately equal to that of water, and if we apply the value of 1370×10^6 km^3 (about 330×10^6 mi^3) as the aggregate volume of the world's oceans (*3*), we have a product of $(5280)^3 \times 330 \times 62.4 = 31 \times 10^{14}$ lb, or 1.5×10^{12} tons. This figure, even omitting the mass of pelogloeal organic matter adsorbed to immersed surfaces, is suggestive of the quantity of suspended organic detritus which, serving as a primary or secondary source of food, is involved in the vast and perpetual metabolic cycles of carbonaceous matter in the sea.

Some approximate values derived from estimations by Riley and others (*17*) would suggest that the foregoing provisional value may not be far out of line. If, for example, a relatively high value, 200 g, of organic carbon (or roughly 400 g of organic matter) may be taken to represent the quantity produced annually by marine phytoplankton beneath each square meter of ocean surface, then applying the factor of 360×10^{12} m^2 for the total ocean surface yields a value of 14.4×10^{16} g, which is equivalent to about 3×10^{14} lbs, or 1.5×10^{11} tons, of organic matter synthesized per year.

The estimated minimal standing crop of suspended organic matter exceeds this quantity by tenfold. Adding the estimated total productivity of land plants would augment the phytoplankton value by only about 3×10^{10} tons per year (*18*), or roughly 17 percent, giving an aggregate of about 1.8×10^{11} tons of organic matter synthesized per year on land and sea.

Since the foregoing value for total annual marine phytoplankton productivity may be somewhat high, and the estimated value for the standing crop of organic matter relatively low (that is, referring only to suspended, not to sedimentary or adsorbed, organic matter) the persistent quantities may exceed the annual synthesis by more than one order and perhaps by as much as two orders of magnitude.

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It is a curious fact that, having minds tending to the infinite, with imaginations unlimited by time and space, the limits of our exact knowledge are very small indeed.
—HENRY A. ROWLAND.

SCIENCE ON THE MARCH

PROCUREMENT OF MONKEYS FOR THE RADIOBIOLOGICAL LABORATORY

THE difficulties experienced by the Radiobiological Laboratory of the University of Texas and the United States Air Force, Austin, Texas, in procuring *Macaca mulatta* (rhesus) monkeys that were free of tuberculosis, excessive parasitism, the effects of malnutrition, and so on, led to the decision to obtain the monkeys in their indigenous habitat where a physical examination could be made on the spot and the transportation of the animals could have the technical supervision of a veterinarian. Therefore, one of us (B. D. F.) proceeded to New Delhi, India, during June 1953 and entered into negotiation with a commercial animal-trapping firm for obtaining 440 *M. mulatta* monkeys. This was repeated in September 1954, this time for 500 additional monkeys.

Rhesus monkeys are found throughout India, but they are most common in northern India where they cause considerable damage to crops and prove to be a general nuisance. The monkeys obtained for the Laboratory were caught in Uttar Pradash, approximately 450 mi west of Dehli.

The Government of India enforces a closed season on monkeys from 1 April through 31 August, although the reasons given locally for this embargo are conflicting and ambiguous. Export for research purposes only is permitted during this period, and then only on presentation of proof of the purpose for which the monkeys will be used. During the open monkey season exports are permitted for zoological gardens, circuses, and so forth, as well as for research, and then no proof of their eventual purpose is necessary.

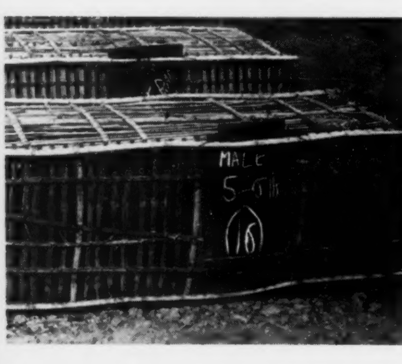
The men employed by the commercial trapping firm are Mohammedans. Because the Hindu religion holds the monkey in veneration, Hindus will not assist in catching them; quite often the Hindu inhabitants of certain villages through which the monkeys have to be transported object to their removal and have, on occasion, attacked the attendants and released the monkeys.

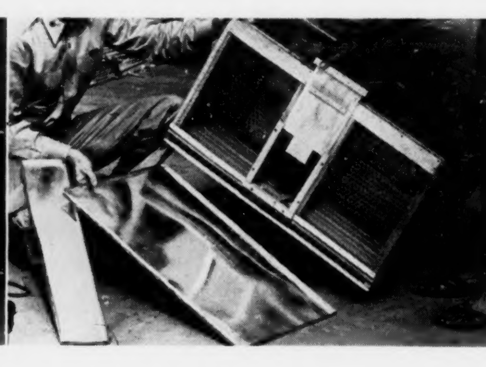
Trapping. All trapping was carried out in the jungle, well away from villages and inhabited areas. It is our observation that monkeys captured in nonpopulated areas have an extremely low incidence of tuberculosis and are invariably healthier than those that have lived in close proximity to man. This suggests the possibility that in India tuberculosis is indirectly contracted by monkeys from man.

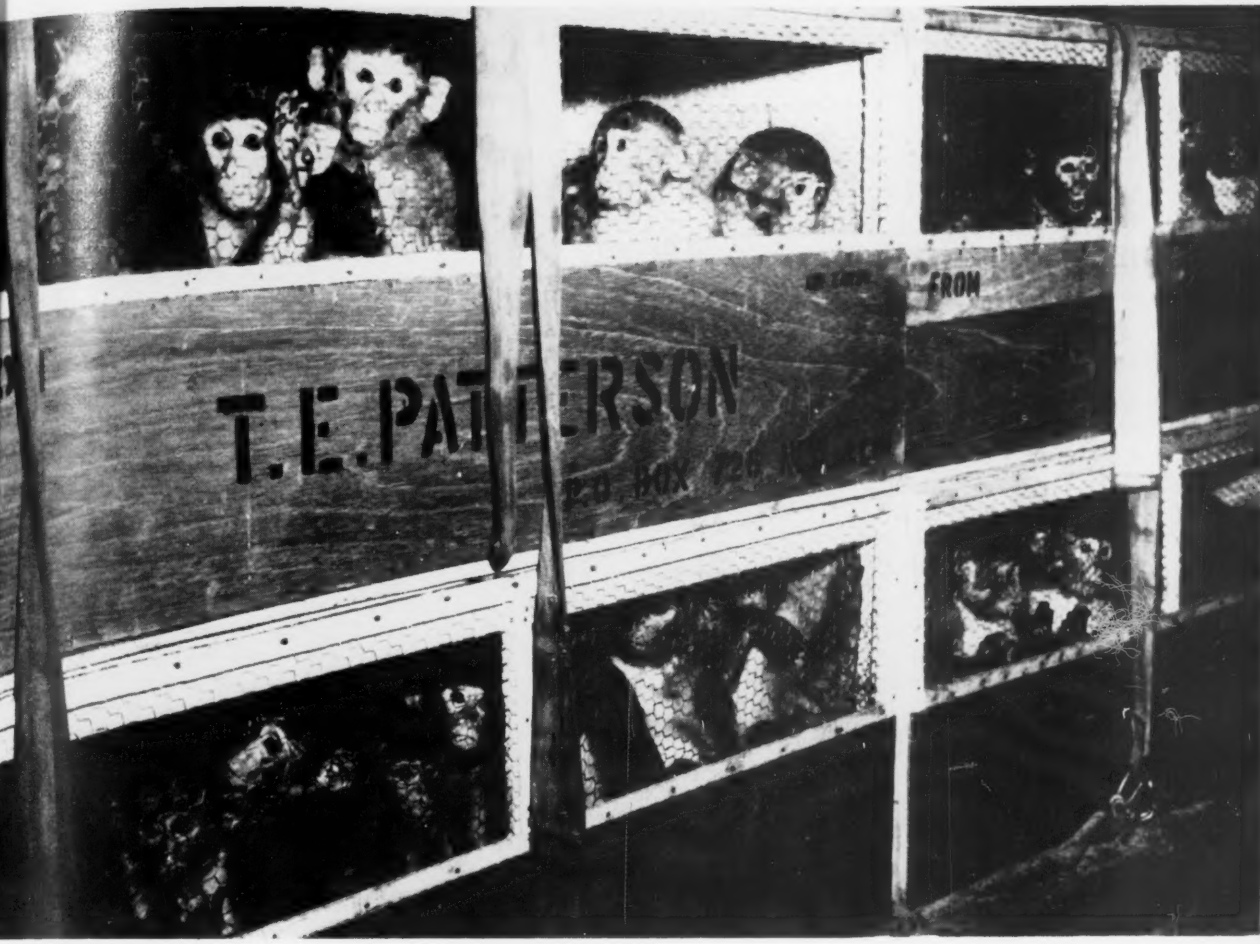
The actual method of trapping monkeys in large numbers is a closely guarded commercial secret. The monkeys' reaction to the sight of members of their troop captured is nearly always marked and prompt. The free monkeys make every possible effort to effect the escape of their captured friends, often attacking personnel handling the monkey traps with vigor. Trapping was not repeated in the same area for a period of 3 mo, since one such experience tends to make the animals wary.

The newly captured monkeys were promptly transferred from the net traps to bamboo cages. These cages were then transported to a temporary jungle camp where the animals were sorted as to size, sex, and condition. When approximately 100 suitable monkeys had been processed, they were shipped via bullock cart or station wagon to the nearest railroad station and immediately transported, roughly 500 mi, to the permanent base camp of the trapping firm in New Delhi.

The monkey transport cages utilized in the movement from the jungle to New Delhi were constructed of bamboo (see bottom left on this page), and were approximately 6 by 3 by 2 ft. A water vessel and two deep feeding pans were included in each cage. Seven to 12 animals, depending on the size of the monkeys, were transported in each cage. During the 2-to-3-day trip to New Delhi, the ani-







mals were fed and watered twice daily. The feed consisted of gram (an indigenous grain similar to barley, the taste of which resembles dried peas), barley, sweetpotato, carrot, and banana.

Health examination and testing. Tuberculin testing of 500 male rhesus monkeys, weighing from 5 to 8 lb, was accomplished at New Delhi by using 0.1 ml of a 1:10 dilution of Koch Old Type tuberculin injected intrapalpebrally. On the first procurement trip one positive and two suspicious reactors to the tuberculin test occurred in the 500 monkeys tested. The tuberculin reactors were eliminated and a careful physical examination was conducted on the remaining monkeys. Four hundred forty of these monkeys were selected for shipment to the USAF School of Aviation Medicine. For the 600 monkeys obtained on the second trip, the same procedure was followed. One positive and one suspicious tuberculin reactor were found, and from the remaining animals 500 were chosen for shipment.

Oral terramycin, 125 mg per animal daily, was administered on three consecutive days prior to departure from India. This prophylactic measure was taken to minimize the incidence of diarrhea while the animals were in transit to San Antonio, Texas.

Transportation. Cages were constructed in New Delhi for the air transportation of the monkeys, with special attention given to sanitation, feeding, and prevention of corrosive damage to the aircraft by excreta (see bottom center on page opposite). Size of the cages was 18 by 18 by 36 in. (6.75 ft^3) with a slatted floor above a tin-plated metal tray. A removable food and water compartment running the length of each cage was also installed. These features reduced to a minimum the amount of animal excreta scattered on the floor of the plane and minimized the labor required to feed and water the monkeys and to clean their cages. Nine to 10 monkeys were shipped in each of the cages (see top of this page).

The precautions taken while transporting the monkey shipments to the United States were as follows: (i) Altitude maintained by aircraft with nonpressurized, nonsoundproof cargo compartment

was limited to not more than 3 hr at 10,000 ft, not more than 4 hr at 8500 ft, not more than 7 hr at 7000 ft, and not more than 15 hr at 4500 ft altitude. (ii) Monkeys were fed twice and watered three to four times during a 24-hr period (see photo lower right on page 260). Gram and barley that had been soaked in water 4 hr were fed to the animals. (iii) Ventilation was limited to good circulation of air without direct drafts on the monkeys. (iv) The temperature of the cargo area was maintained at between 65° and 80° F, and sudden changes in temperature greater than 30° F were avoided. (v) Agitation of the monkeys while in transit was kept at a minimum; for example, no teasing by personnel was allowed. (vi) The monkeys' cages were cleaned once every 24 hr.

Three animals died in transit from India to the United States owing to heat exhaustion that resulted when an engine failure forced the plane to be grounded for 3 days in the intense heat of Saudi Arabia. On the second trip one animal died in transit from undetermined causes.

The loss of animals in the first air shipment amounted to 0.7 percent, that in the second shipment amounted to 0.2 percent, as compared with a loss of 6 to 40 percent in shipments via boat as reported by Carpenter [*Science* **92**, 284 (1940)].

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BOOK REVIEWS

The North American Prairie. John E. Weaver. Johnsen, Lincoln, Nebr., 1954. xi + 384 pp. Illus. $5.

THE department of botany at the University of Nebraska shares with its sister department at the University of Chicago the honor of launching plant ecology in the Western Hemisphere. John E. Weaver, professor emeritus of that subject at Nebraska, in a beautifully printed and illustrated book, now sums up the results of a lifetime spent in studying the prairie. Such an event happens too infrequently in science. It is all the more noteworthy in this instance because the work of Weaver and his students has profound implications for the economic and esthetic welfare of a great continent.

A vast portion of the interior of North America had, as its natural vegetation, the grassland. The more humid easterly portion of this grassland was prairie, variously known as true prairie or tall-grass country. Actually it contained many species that were not grasses, notably legumes, that accounted for the high nitrogen content and consequent fertility of its soils. The dark humus horizon in these soils, from 3 to 5 ft deep, expressed the interaction of masses of fibrous roots, microorganisms, and burrowing animals and a climate in which rainfall and evaporation were nicely balanced, despite the dryness of late summer and the occasional recurrence of severe drouths. To these vicissitudes, however, the rich community of species was adapted by a process of natural selection that has been under way since the Tertiary.

No matter what the season, this resilient living system produced organic material to sustain itself and the animal life dependent upon it. Unhappily, it has not been able to defend itself against the final hazard of modern man, although potentially it was his friend. Its fertile and friable soils have become his granary. Where he has cherished the native vegetation with anything like reasonable care, it still affords prime pasture for his livestock. During the drouth of the 1930's, I knew of several districts in which the only available forage for cattle was the hay and grazing from prairie that, by some miracle, had escaped the plow. One of the features of this book is the lucid demonstration of the direct utility of the native prairie and of its superiority to the degraded secondary vegetation that replaces it under undue grazing pressure.

Weaver begins with descriptions of the grasses and other herbs of lowland and upland prairie. These are mildly technical but readable accounts of the more important plants, and they are illustrated with superb plant portraits. Fewer than a dozen major grasses are involved out of nearly 300 species in the typical prairie flora, and chief of these are the bluestems, big and little. A knowledge of these grasses and their behavior is as essential to the stockman as a knowledge of trees is to the forester.

The author then introduces us to an invisible empire —the underground domain of infinitely varied root systems and hidden stems that anchor, supply water and minerals, store food reserves, and bind the soil into firm sod. This is the difficult and muscular aspect of botany in which, through 40 years, Weaver and his students have done some of their most notable work. Later in the book, in a striking set of pictures, we are shown how the progressive enfeeblement of a prairie under abuse is reflected in the diminished vigor of its root systems.

There are two absorbing chapters dealing with what might be called "the prairie at work." Here we learn of its dynamic stability, its changing aspects through the year, its response to environment, and its endless cycle of growth, reproduction, decay, and renewal.

The final portion of the book, six of its 16 chapters, tells of the prairie under attack. There is a brief account of early descriptions of the prairie and of Euro-

pean settlement, and then firsthand descriptions of prairie remnants from the Dakotas to Texas and from Ohio to central Nebraska. After this, in an account whose dramatic contrast is heightened by the matter-of-fact simplicity of its telling, the effects of two great types of disaster—drouth and modern man—are set forth.

From the 1930's on, Weaver has recorded 'carefully"—to use one of his favorite words—the changes that accompany drouth and its passing. To this assault of nature the prairie responds, not by giving up the ghost, but by a change of composition, mobilizing its reserves of drouth-enduring species to take over during the emergency. When that is past, the usual dominants march back and gradually resume control.

Against so-called "civilized man," however, it has no such defense. To his fires it responds mainly by reducing its yield, for fires, like drouth, have been usual events in its adventurous past. His domestic animals, on the other hand, unless managed with rare finesse, bring degeneration and his plow brings extermination. These changes are described in clinical detail, and yet even here we learn how, if given a chance, the prairie will bring itself back from the edge of destruction.

The mere announcement of this book will insure its being read by ecologists and those interested in range management. It deserves wider reading, however, and was so intended. The amateur naturalist who has a glimpse of the pictures will assuredly get on into the text, which is clear and direct. Others who are concerned about man's relationship to the natural world of which he is a part may have to stretch their vocabularies a bit here and there (*anthesis* means flowering, for example, and a *forb* means an herb that is not a grass). But we do have dictionaries, and it is a poor soul indeed who is afraid of an occasional new word. All who love the prairie owe thanks to Weaver for this book.

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The Judgment of History. Marie Collins Swabey. Philosophical Library, New York, 1954. x + 257 pp. $3.75.

THE author divides written history into three types: "common sense," "scientific," and "philosophical." Common-sense history is described as the older type which is more narrative, more concerned with temporal sequence and persons, and "without a clear-cut enveloping frame" (p. 29) to link it together. Gibbon's *Roman Empire* and Macaulay's *England* are examples that are analyzed.

Scientific history is depicted as the analytic type written more often in the last half of the 19th century and the first four decades of the 20th century. Buckle's *England*, Henry Adams' *Education*, and Beard's *Economic Interpretation of the Constitution* are each discussed at length to illustrate the great influence of Darwin and other natural scientists on the writing of history. Although such influences as greater objectivity and more critical use of documents are mentioned, the author emphasizes others: claims of ability to predict with scientific accuracy; claims of biologic parallels in history; and the purported pattern of survival of the fittest as given by communist, fascist, and imperialist historians. One conclusion reads that the "dynamic blend of Hegelian dialectic and Darwinism has provided the greatest ideological threat of our time" (p. 180). I am of the opinion that the author has not done justice to the many histories which, although written "under scientific influence," have none of the characteristics emphasized. The conclusion is that history written in "purely existential terms, presenting man as wholly a part and product of nature" (p. 253) is unsound and that mankind has now turned back to a conception of a universal law of reason.

Philosophic history, which is preferred, recognizes that the influence of ideas is fundamental, "permeating men's thoughts, constitutions, arts, cultures, religions, and driving them forward. . . ." (p. 174). The illustrations of this type of history which grasps "a larger meaning, a fullness, a grandeur of things" (p. 184) are the works of Parkman and of Whitehead, particularly the latter's *Adventures of Ideas.*

This is a carefully written and thought-provoking, though repetitious, volume, written by a professor of philosophy. It will arouse controversy among historians and also will be of interest to natural scientists with a bent for history or philosophy.

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The Story of Man. From the first human to primitive culture and beyond. Carleton S. Coon. Knopf, New York, 1954. xii + 437 pp. Illus. + plates. $6.75.

AMONG anthropologists Carleton Coon is well known for the diversity of his activities, the encyclopedic sweep of his interests and knowledge, his adventurous and unconventional mind, and his ability to write cleanly, with an absence of jargon and with style. This book brilliantly reflects all these qualities.

It is unfortunate that in their advertising the publishers refer to *Gods, Graves, and Scholars* and to *Man, Time and Fossils.* For, aside from the fact that all three books are written primarily for the public, *The Story of Man* has nothing in common with the other two, which merely recount the high points in the work of pioneers in the study of cultural and biological evolution. Coon is a major anthropologist, with firsthand contributions in physical anthropology, archeology, ethnology, and theory to his credit; and *The Story of Man* is his synthesis of these fields as he appraises man's biological and cultural history to the present day.

His major theme concerns the relationship between man's increasing use of energy and the resulting development of his cultural complexity, including its transformation into social structure. Since he does not reduce this equation to formula, his points are well made and convincing. The human story thus falls into

three major phases with a fourth one just beginning. The first saw the emergence of man as man; the second witnessed the development of techniques by which, as a hunter, man could and did spread over most of the world; the third watched him produce his own food and perfect technical processes which brought the rapid changes leading to the present situation of wide cultural diversity; and the fourth stage, just starting, for the sake of human survival now requires that cultural unification be achieved.

Although the shortest in elapsed time, the third phase is the best documented, the most complex, and accordingly receives the most extended treatment. Coon divides it into eight ages, the Neolithic, Bronze, Iron, Gunpowder, Coke, Oil, Hydroelectric, and First Atomic, which correspond roughly to the stages of the Paleolithic and Mesolithic. He is acutely conscious of cultural lag and of Neolithic patterns of human relationships that survive in industrialized societies.

> One wonders how the transition will be made in Europe, India, and China. . . . Neolithic culture is much more than a subject of inquiry by prehistorians. Moving out of it may be the world's most difficult problem.

No one will agree with everything that Coon says, for he deals boldly with many controversial matters. However, I regard my own criticisms and disagreements as minor. *The Story of Man* is a fresh and unhackneyed view of human history and a penetrating commentary on man's present situation. In looking over the particularly trenchant passages I had marked for possible quotation, I discovered that they take up a good share of the volume. In a brief review such as this I cannot possibly communicate what is in this book; I can only tell you to read it.

Robert W. Ehrich
Department of Sociology-Anthropology, Brooklyn College

Indian Corn in Old America. Paul Weatherwax. Macmillan, New York, 1954. ix + 253 pp. Illus. + plates. $7.50.

ALMOST exactly 40 years before the appearance of this beautiful and interesting volume, Paul Weatherwax began his technical studies of the maize plant and its relatives. Then, as now, he was richly endowed with common sense and a rare ability to go quietly about his own concerns, undisturbed by the scientific fads of the moment. It was evident to him that since maize is a grass, it must be studied as a grass; that until we understand the simple facts about the morphology of maize and its relatives we shall not have the proper basis for other kinds of research. He proceeded with a series of sound technical studies on maize inflorescences, studies that were only tardily recognized by most American students of maize, although they were immediately appreciated by the world's leading agrostologists.

The present volume mentions these earlier technical studies only in passing. It is largely concerned with the role that maize played in the life of pre-Columbian America. It is in Weatherwax's own words an

> . . . endeavor to reconstruct a picture of the corn plant and the system of agriculture and household arts based on it as they existed in ancient America, with some consideration of the part that corn played in the everyday life, the thinking, and the artistic expression of the peoples of this hemisphere before Columbus came.
>
> To sharpen the focus we shall deal largely with the American agricultural scene as it appeared in the sixteenth century—with a picture of what the early Spanish explorers saw or might have seen if they had made a rapid journey over this vast and exciting new country soon after it was discovered. To secure a proper setting, it is necessary to go back a little way in time and inquire into the botanical origins of the plant and the ways in which the Indians had been dealing with it previous to 1492, and equally important that we take a brief look forward to its introduction into Europe and the beginnings of its assimilation into the newly born western civilization.

The resulting volume is an attractive book about the size and shape of an old-fashioned geography textbook. It opens nicely in the hand and displays effectively its 75 plates, most of them full page and three of them in color. There are footnotes for every important reference, a bibliography of cited works, and a complete index.

It is written throughout in a good, honest, understandable sort of professorial prose. Such technicalities as chromosomes are mentioned only when they are really pertinent to the discussion and even then are skipped over lightly. My own voluminous writings and those of my students are pretty much ignored along with other studies published in the last decade, occasionally even when they bear directly on the point at issue. Most of these omissions are quite as they should be. Although the papers of Alava, Lenz, and Nickerson, for instance, are concerned with matters upon which Weatherwax is one of the world's few authorities, these technical details would be as out of place in such a volume as most of Weatherwax's own studies in this field, which are likewise omitted. For the few which logically might have been included (Wellhausen's magnificent memoir on the *Races of Mexican Maize*, Brown's treatise on the *Maize of the Caribbean*, my own studies with Isabel Kelly of the popcorns and sweet corns that she discovered in Mexico), those who know Weatherwax will have a likely explanation. He is much too well-integrated an individual to be affected by pique; it is rather that a scholar sturdily independent enough to go his own way for nearly three decades will quite naturally be a little slow to seize upon the contributions of other minds when they at length join in the quest which for so long was virtually his alone.

Edgar Anderson
Missouri Botanical Garden, St. Louis, and
Henry Shaw School of Botany, Washington University

Graphic Problems in Petroleum Geology. L. W. LeRoy and Julian W. Low. Harper, New York, 1954. ix + 238 pp. Illus. + plates. Paper, $4.50.

THIS excellent manual is designed for the training of advanced college students in petroleum geology. Recognizing the need for careful and thorough training of geology students in the fundamentals of the science as well as in the many and varied graphic techniques employed by the petroleum industry, the authors have prepared a manual that provides training in the solution of basic problems by graphic methods.

The volume includes a general discussion of graphic problems and a short chapter on drafting materials and instruments, but the major portion of the book consists of 31 problems to be solved by the student. Many of the problems have several parts. The questions which accompany each problem are designed to call the attention of the student to the specific geologic interpretations that can be made from the drawings he prepares.

As is suggested by the authors, it may be found that different parts of the manual may be used in several courses because some of the problems are more difficult than others and because some deal primarily with structural geology while others are concerned with stratigraphy or sedimentation.

TRAVIS J. PARKER

Department of Geology,
Agricultural and Mechanical College of Texas

Introduction to Vertebrate Embryology. Waldo Shumway and F. B. Adamstone. Wiley, New York; Chapman & Hall, London, ed. 5. 1954. x + 389 pp. Illus. $6.

A Laboratory Manual of Vertebrate Embryology. F. B. Adamstone and Waldo Shumway. Wiley, New York, ed. 3, 1954. vi + 98 pp. Illus. $2.50.

SOME additions and corrections characterize this new edition of a familiar textbook. There are only minor rearrangements in the organization, and few changes in the illustrations. The primary effect of the revision has been to modernize the contents. The chapter on experimental embryology, not very much changed since the last edition (1942), is now promoted to stand as chapter 10 (out of 22) from its former position as chapter 17 (out of 19). Since the authors have incorporated many of the data of experimental embryology into the body of the textbook, where they perform their only rightful function, that of elucidating the processes of normal embryology, it is a pity that the name of the chapter remains unchanged; it suggests a dichotomy between "experimental" embryology and "other" embryology which no longer in fact exists. Also, some anachronisms still remain with respect to the inclusion of new experimental material. The retaining, for instance, on the page facing Spratt's newer diagram of the Lillie diagrams suggesting the formation of the primitive steak of the chick by concrescence may be well advised from the historical point of view, but it is of questionable pedagogic value with the text as it now stands; the students will "like" and "understand," and therefore remember, the apparently simpler diagrams of concrescence. Such objections, however, will probably seem trivial to all but a handful of experimental embryologists. And this essentially sound and well-conceived textbook has been greatly increased in usefulness by being brought up to date.

In the *Laboratory Manual* fewer changes have been made. The section on the frog has been altered slightly to bring it into conformity with that on the chick, and the sections on gastrulation and formation of the germ layers in frog and chick, already well done in the previous edition, have been amplified and improved in this one. The manual is an extremely serviceable one, and is strongly recommended for use not only in conjunction with the same authors' textbook, but also for courses which (like mine) assign no textbook, or which use those by other authors.

JANE OPPENHEIMER

Department of Biology, Bryn Mawr College

Africa Drums. Richard St. Barbe Baker. George Ronald, Wheatley-Oxford, England; British Book Centre, New York, rev. ed., 1954. 159 pp. Plates. $3.

THE author has had a distinguished career. He is a veteran of World War I, a professional forester, a world traveler and lecturer, a former assistant conservator of forests in Kenya and Nigeria, and the founder of The Men of the Trees society and of the Forestry Association of Great Britain. Among the books he has written are the autobiographical *I Planted Trees*, *Green Glory*, *The Men of the Trees*, and *Trees—A Book of the Seasons.* He has long been concerned with world deforestation and is credited with having given Franklin D. Roosevelt, when the latter was governor of New York, the idea that culminated in the formation of the Civilian Conservation Corps.

Africa Drums first appeared in 1942. The outstanding impression is that the author's heart resides in Africa. He is the first and only white man to be initiated into the Kiama, a secret order of blood brotherhood of native Africans. His affection for, understanding of, and interest in the aborigines of "the dark continent" form the connecting thread of the entire narrative, which is an entertaining and instructive potpourri of geography, ethnology, anthropology, sociology, and forestry. There is a moving description of his first contact with African lepers, until then wholly neglected, and of his final success in obtaining chaulmoogra-oil trees for them.

A few chapters deserve special mention. "Drums and television" tells the story (often weird) of native communication. "Nigerian magic" and "Under the spell of ju-ju" deal with indigenous occult mysteries. In "African heroes" the author pleads for a more sympathetic recognition and appreciation of African tradi-

tions and legends, for an effort to preserve these in written form, and for less emphasis on replacing African with European culture. Some of these African legends are briefly retold. "The mighty bowmen" describes a forgotten race which Baker believes is descended from an early African aristocracy. He has much to say of native customs and the love of dancing; the latter he put to practical purpose by instituting "The dance of the trees," wherewith native interest in afforestation was aroused.

Scattered throughout are items of interest to the professional forester—the introduction of the African juniper (*Juniperus procera*) into the pencil trade; management of the so-called "mahogany" forests; and the importance of natural areas in understanding the basic principles whereby a managed forest should be operated. The author is a skilled narrator and projects a warm personality through the black-and-white characters of the printed page. The 48 halftones from his photographs are excellent.

WILLIAM A. DAYTON
Forest Service, U.S. Department of Agriculture

Back of History. The story of our own origins. William Howells. Doubleday, Garden City, N.Y., 1954. 384 pp. Illus. $3.50.

WILLIAM Howells has scored another hit. Here is his best work yet, a readable book of up-to-date factual anthropology, and with it a delightful and vivid account of man's long struggle on this earth. Here, clearly set down, are the essentials of our human background (human biology coupled with sociocultural progress) that everyone should know. The same easy manner of deft expression which characterizes Howells' other books *Mankind So Far* and *The Heathens*, will prove *Back of History* to be even more popular.

It is conveniently divided into six chapters followed by an appropriate and interesting epilogue, the author's note, and a good index.

Howells spins a remarkably apt story—informative, delightful, and different. Its world-wide scope is more comprehensive than either V. Gordon Childe's *What Happened in History* or *Man Makes Himself*. Eye-catching, appropriate, and easily remembered phrases in typical "Howellese" (such as "the Pleistocene back-drop," the Piltdown prank," "the Mesolithic masters of the hunt," "the first food growers," "the Neolithic," "the last living hunters") bring a fresh lively touch to this book.

The neat attractive format interrupted by few footnotes is appropriately punctuated with new, effective line drawings of flint tools, fossil skulls, pelvic bones, pots, buildings, early writing forms, and various maps showing the distribution of culture areas, migration routes, and racial groups.

After a fascinating account of various primate societies, Howells clarifies our own evolution, "For the story of man is a nature study," and sets us traveling on his tour in the wide realm of the world's peoples and their culture. It is significant, too, that his remarks on stone tools precede those on early men. It is not organic evolution alone that so separates mankind from all other creatures; it is what he makes and thinks and does!

Perhaps this part (p. 44) will show the style and approach of Howells:

> We can hardly understand human social relations until we see that our basic social institutions are imposed over a powerful set of natural inclinations, derived through our biological evolution to act in the very way we act. The other Primates have shown us what these inclinations are: The need of individuals to be in a society and to establish secure, specific and complex relationships with one another.

Howells has written a popular book, well suited not only for fellow social scientists and other students of humanity but particularly for the lay reader. Indeed, most people should find this an excellent guide toward a truer understanding of and an appreciation for all the world's people. For some, the glib fundamental story may be too elementary, too general. Howells recognizes this responsibility and states his position thus: (p. 364)

> I believe the ideas I have set forth are generally near the center of gravity of my colleagues' present opinion.

Believe me, most of his colleagues will get this book, and all anthropology majors should find it a most worthwhile guide.

CHARLES E. SNOW
Department of Anthropology, University of Kentucky

General College Chemistry. M. Cannon Sneed, J. Lewis Maynard, and Robert C. Brasted. Van Nostrand, New York, ed. 2, 1954. vi + 693 pp. Illus. $6.50.

THIS new edition is a thorough revision, particularly of the material on atomic structure, nuclear reactions, and industrial chemistry. Material on organic chemistry has been added that will be of particular interest to those who intend to major in bacteriology, home economics, nursing, medical technology, and other related fields. The new edition uses the system of nomenclature recommended by the Committee of the Internation Union of Chemistry for the Reform of Inorganic Chemical Nomenclature. The authors are to be congratulated for achieving these changes in the fewer pages of the new edition.

Atomic structure is developed in the second chapter via radioactivity, spectroscopy, and elementary quantum theory to the *s, p, d, f* orbitals of the principal quantum levels. The introduction to the concept of valence is beautifully done in clear, concise language. The treatment of ionic equilibria and electrochemistry is especially noteworthy. Although the treatment of fundamental theory is modern and thorough, the descriptive chemistry of the elements is in no sense slighted. In short, the new edition is a well-balanced textbook that can be highly recommended.

JOHN A. TIMM
Department of Chemistry, Simmons College

Fundamentals of College Mathematics. John C. Brixey and Richard V. Andree. Holt, New York, 1954. xiv + 609 pp. Illus. $5.90.

THIS textbook is designed for a first-year course in college mathematics. It presents topics from algebra, trigonometry, statistics, plane and solid analytic geometry, and the calculus with a happy combination of carefully written, readable text and interesting problems.

Throughout, the authors endeavor to develop the student's understanding of concepts that are necessary in more advanced work in mathematics and related fields. For example, they emphasize the analytic portions of trigonometry rather than the solution of triangles. They are well aware of the difficulties that new notation often puts in the way of the understanding of a new concept and they recognize the value of presenting a subject in such a way that foundations are properly laid for further work in that subject. As cases in point, the summation notion is made familiar well in advance of the chapter on statistics; special devices for evaluating determinants of the third order are avoided.

The book is adaptable for various types of courses, short courses in college algebra and statistics or in modern trigonometry, a course in analytic geometry with or without solid analytic geometry, a two-semester course covering the major portion of the text, or, to quote the authors, "terminal courses providing mathematical background for liberal (or general) education." The final chapter enriches the book by dealing with selected topics from more advanced mathematics.

The numerous illustrative examples and problems are well selected. Occasionally the intrinsic value of the latter is marred by a studied cleverness of presentation. In the rather fully outlined solutions of many of the odd-numbered problems and in the wide margin on each page the book presents two unusual features, potentialities for good or ill use.

The authors have added to the textbooks on introductory college mathematics a contribution which favorably reflects their own classroom experience and should prove of genuine worth in the classrooms of others.

HELEN G. RUSSELL
Department of Mathematics, Wellesley College

General Chemistry. Edwin C. Markham and Sherman E. Smith. Houghton Mifflin, Boston, 1954. x + 613 pp. Illus. $6.

THIS textbook presents a veritable "smörgasbord" of chemical principles and facts, arranged so as to make selection easy and convenient. Its two-column format and the unusually large number of interesting drawings and photographs make it attractive. A good list of references for additional reading is included.

The order of topics will appeal to teachers who prefer their students to study the essential ideas concerning the structure of atoms and molecules before taking up the conventional descriptive material. "Water" is discussed in Chapter 19, "Oxygen" in Chapter 21, and "Hydrogen" in the middle chapter of the book.

There are several errors besides the 37 listed on a supplementary sheet. Some are typographical errors (p. 300: "Fluorine was first prepared . . . in 1806"), some result from lapses in the authors' attempt to show the charges for all electrovalent compounds (p. 457: "$Ba^{++}O_2^{--} + H_2SO_4 \rightarrow BaSO_4\downarrow + H_2O_2$"), but some represent genuine misunderstandings. I disagree with such statements as "on passing through the electrical field . . . ions bearing single charges emerge . . . at a nearly constant speed" (p. 112) and "nitric oxide . . . is dissolved in rain and carried to the earth" (p. 287).

Most of the mental food in this textbook is so thoroughly chewed and predigested that the student is rarely required to do any original thinking. Many of the questions at the ends of the chapters can be answered by a direct quotation from the textbook. Verbalism, rather than the development of scientific habits of thought, is encouraged by questions such as "List the inert gases in the order of their decreasing abundance in the atmosphere."

FRANK D. MARTIN
Department of Chemistry, Purdue University

The Kachina and the White Man. A study of the influences of white culture on the Hopi Kachina cult. Frederick J. Dockstader. Cranbrook Institute of Science, Bloomfield Hills, Mich., 1954. xiv + 185 pp. Illus. + plates. $5.

THE colorful dances of the masked Kachina impersonators in the villages of the Hopi Indians of Arizona have intrigued American visitors for three-quarters of a century. The mystic beauty of exotic costuming, combined with elaborate religious symbolism in the dramatic performance of the many dances that occur during the winter half of the year, give expression to the Hopi's sense of oneness with the spirit beings who once lived with the Hopis and then departed for the underworld. But the Kachinas, though departed, are still the benign well-wishers who return annually through the masked figures of the Hopis themselves to bring happiness and prosperity to the desert villages.

The earliest Spanish visitors to Tusayan, the Hopi country, paid little attention to the pagan rituals and left only the slightest mention of the masked dances. Extensive archeological work in the Southwest has recovered no prehistoric Kachina masks. Hence, the view spread among ethnologists that the Kachina cult is a post-Spanish innovation stimulated by the Mexican *matachina* dances of Spanish origin.

Dockstader is strongly entrenched in the opposing camp. He marshals arguments both negative and positive to demonstrate that the Kachina cult is indigenous to the Pueblos. The reader who is not a Southwestern specialist is not likely to get very concerned with the

outcome of the issue, but he will enjoy Dockstader's book. The opening chapters on the Hopi world and the Kachina cult are written with fine empathic feeling and understanding in stylistically excellent prose that matches the striking color representations of Kachina costumery drawn and painted by the author himself. Although the literary interest of the book sags as the author presents the technical details of artifactual evidence bearing on the question of the origin of the cult, its compelling quality is recaptured as he moves on into the social history of Hopi vicissitudes with conquistadores and padres, their period of relative freedom from interference (1700–1875), and into the intolerant and foolish era of American suppression of Indian rights of cultural freedom (1875–1935). The effects of a good deal of acculturation are skillfully compressed into a relatively small number of pages.

The craftsmanship that has gone into the production of *The Kachina and the White Man* matches the effectiveness with which the author has treated his subject. The result is altogether pleasing; it will give satisfaction to anyone interested in comparative religion and ceremonial drama, in culture history, or, more specifically, in Hopi Indians.

E. ADAMSON HOEBEL

University of Minnesota

Proceedings of the Seventh International Botanical Congress. Held in Stockholm, 12–20 July 1950. Hugo Osvald and Ewert Åberg, Eds. Almqvist & Wiksell, Stockholm; Chronica Botanica, Waltham, Mass., 899 pp. Plates. $17.35.

THIS large volume, well printed in double-column format, is a complete account of all the proceedings of the Seventh International Botanical Congress. Excellent abstracts of most of the papers presented before the sections, as well as discussions of each, are included. The sections were "Agronomic botany," "Cytology," "Experimental ecology," "Experimental taxonomy," "Forest botany," "Genetics," "Morphology and anatomy," "Mycology and bacteriology," "Paleobotany," "Phytogeography," "Phytopathology," "Plant physiology," and "Taxonomy." Discussions of proposals for modification of the Rules of Botanical Nomenclature, presented to the Section on Nomenclature, are given in detail, and also the result of the vote on each proposal. Four supplements to the proceedings of the Section on Nomenclature include reports of special committees on the type method, nomenclature of cultivated plants, paleobotanical nomenclature, and nomenclature of fungi. The several excursions, before, during, and after the congress, are described, and the participants listed. Some of the interesting events of the congress are illustrated by 20 fine plates. The chief editor, Hugo Osvald, and his committee are to be congratulated for preparing such an adequate and interesting account of the congress.

JASON R. SWALLEN

Smithsonian Institution

History of American Industrial Science. Courtney Robert Hall. Library Pub., New York, 1954. xix + 453 pp. $4.95.

IF we go beyond the "good old days" to America's beginnings, we find few indications of what this country was to become industrially. There is much hope in the scattered optimistic analyses of commercial companies who planted the first colonies, and some instruction in the conservative writings of philosophers and ingenuous tinkerers who took time out from farming to engage in simple home manufactures and to assess the wealth around them. Yet, being a historian, this is where Hall begins his story of American industry. "Science" is conspicuous by its absence. Within a few pages he covers the Revolutionary War, the industrial revolution and the beginnings of textiles in New England, iron goods in Pennsylvania, and clocks, brass, shoes, and firearms at sundry locations.

The industrial state really began, he says, with the Civil War. From there he carries us through the heyday of railroading, carbon electric bulbs, biplanes, wireless, and early jet planes to the Atomic Age. He ends with bare mention of such things as double-weight hydrogen, tritium, the hydrogen bomb, the first atomic submarine, and commercial use of atomic energy. His principal eyepiece for viewing the future is the Paley Report, with its forecast of tremendous drains on our raw materials and a half-trillion-dollar economy by 1975.

Hall says

> Much of the material which would inform us about industrial development is garbed in a form which is clear only to the technically trained specialist. . . . An air of mystery seems to pervade the great factories and laboratories in which the miracles of modern industrial science are performed.

His one purpose in this book

> is to help make the general public aware of the need for continued improvement of our industrial system . . . and to tell the story in terms most people can understand.

He bulks together as "industrial science" all fundamental science, applied science, research, engineering, invention, and fortunate accidents that had some part in the progress of American business. He seems to have missed entirely the importance of the vital period from about 1820 to 1850 when natural philosophy was blossoming into natural science and when many deep thinkers were organizing academies and scientific societies! At the same time, technical and engineering schools were being founded and new scientific disciplines were rising.

Lacking new lands to exploit, industrial scientists are America's primary sources of new wealth. Hence an astonishing gap is the book's failure to recognize the contributions of company-sponsored scientific programs. Only a few of the independent industrial research laboratories are mentioned, and there is no discussion at all of the American patent system, the National Bureau of Standards, nor the photographic industry.

However, the book is a start toward providing a

needed reference on American business history and serves to focus attention on scientific planning and research.

We can forgive the omissions of a pioneer who is working practically alone along a new trail, but more will be expected of future efforts to write a comprehensive history of American industrial science. Meanwhile, this volume will give the youthful student hasty glimpses into the beginnings of many products and many businesses. It will provide the casual reader with irrefutable evidence that science is an important avenue to better business, more jobs, and added opportunities for happiness.

C. GUY SUITS

General Electric Research Laboratory

Nomography and Empirical Equations. Lee H. Johnson. Wiley, New York, 1952. ix + 150 pp. Illus. + charts. $3.75.

THIS work of Lee Johnson performs a useful service in placing before the technical public a clear and simple explanation of the theory and construction of nomographic charts. These alignment charts for the solution of various algebraic equations are familiar to most scientists and engineers, but the simple methods for constructing them are by no means as widely known. I had always viewed the construction of a nomogram in much the same way that one would consider the construction of a crossword puzzle—simple when accomplished, but probably maddening to devise. Johnson, however, brings forth lucidly that this is not the case. The theoretical background of nomograms and the specific solutions possible for a variety of functional relationships are fully and clearly described.

This book makes a useful reference for the practical engineer or scientist; its main purpose, however, is a little cloudy. Purely as a reference, it seems somewhat lengthy, with the simple mathematical background repeated perhaps once too often. A tabular summary would have made this book more useful as a reference. Somewhat the same criticism applies when it is considered as a textbook. As part of a wider course in, say, engineering analysis, it would be useful, but there does not seem to be enough substance around which a college course could be built.

The second half of this book (which seems rather oddly paired with the first half) is concerned with the deduction of empirical equations from experimental data. Here again, it is valuable as an aid in solving particular cases, the tabular summary at the end of this section being of particular utility. This part is somewhat more powerfully written than the first section, but makes rather heavy reading. With the aid of this book, the art of reducing experimental data to algebraic form can be transformed into a somewhat more scientific process, and, as such, Johnson has performed a practical service for the engineer and experimental scientist.

K. DEXTER MILLER, JR.

Province Line Road, Princeton, New Jersey

Nobel Prize Winners in Physics: 1901–1950. Niels H. de V. Heathcote. Schuman, New York, 1953. xvi + 473 pp. Illus. + plates. $8.50.

BETWEEN 1901, the year when Wilhelm Konrad Röntgen received the first Nobel prize in physics, and 1950, there were 53 other physicists recognized. Niels Heathcote, lecturer in history and philosophy of science at the University of London, has presented an exposition of the principal facets of the work of each of these persons. Here is a good place to find suitable extracts from the original papers delivered by the scientists themselves when each appeared to receive his award in Stockholm. Heathcote has prefaced each man's work with a brief biographical note and a sketch of the relationship of the work to physics generally. A few restrained notes on the consequences of the work and on its importance in subsequent years are added. In these respects, the book is of value as a reference. Both the extracts and the comments are necessarily brief.

Perhaps it is because of its brevity and the need to omit what might have seemed trivial to the author that the book falls short of conveying the full sense of the drama of scientific discovery. All 54 winners are accorded a factual, level treatment. With some exceptions, I find the book weak in what might be called "interpretation." Without descending to the level of science fiction, a writer with such rich subject matter and personalities to draw upon could conceivably have made a more interesting book, a book in which the vicarious thrill of great discovery would be better conveyed.

RICHARD M. SUTTON

Department of Physics, Haverford College

Saipan: The Ethnology of a War-Devastated Island. Alexander Spoehr. Chicago Natural History Museum, Chicago, 1954. 383 pp. Illus. Paper, $5.

THE Pacific islands taken over from Japan in World War II, now a United States trust territory, have been the scene of one of the most spectacular and successful experiments in collaborative research that science can count to its credit. Organized by the National Research Council's Pacific Science Board, it has involved the participation of many institutions and a wide range of scientific and applied research fields. Alexander Spoehr, now director of the B. P. Bishop Museum in Honolulu, and formerly curator of oceanic ethnology at the Chicago Natural History Museum, has carried out two of the anthropological projects. The first, an ethnological survey of a Marshall Islands community in the wake of the war, has become part of the standard record (*Majuro, a Village in the Marshall Islands*, 1949). This new volume is one of the reports from a further field study conducted in the northern Marianas, and covers the ethnology of the present day "Chamorros" and of migrant "Carolinian" peoples resident on Saipan island.

The devastating bombardment of the war period was

by no means the only traumatic experience undergone by the Saipanese. As Spoehr shows, the Spanish colonial regime had included forcible removal of the population to Guam for well over a century. Spain's influence, along with extensive Hispanization, brought the Chamorros to near extinction, and the modern people represent a mixture of the survivors with diversified immigrant elements. Germany took over political control in 1899, and from 1914 Japan made Saipan a focus for Japanese settlement and industry. With American occupation, the local people, numbering close to 5000, were held behind barbed wire in one consolidated settlement by military government authorities. Later, the Navy assumed responsibility for civil administration, then the Department of the Interior, and now the Navy once more. The Saipanese indeed had to be adaptable to survive.

Spoehr's major focus is on the contemporary cultural systems of the Chamorros and Carolinians, with close to half the work devoted to the analysis of kinship. Above all, it is a vital casebook on "cultural dynamics." He shows, on the one hand, how the Roman Catholic faith has taken hold along with much else from the West, and how malleable even if uncertain have been the economic and political aspects of life, yet, on the other hand, how persistent are, for example, the Chamorro language, much of kinship, and many of the older beliefs and related practices. The volume is also a model of modern ethnological reporting: lucidly written, with excellent maps, diagrams, and photographs, and a full bibliography.

FELIX M. KEESING
Department of Anthropology, Stanford University

Community and Environment. A discourse on social ecology. E. A. Gutkind. Philosophical Library, New York, 1954. xiii + 81 pp. $3.75.

THE introduction gives the purpose of this book:

> In this essay I have tried to sketch in broad outlines the main problems and a few methods which in my opinion should be taken into account in working out a framework of reference for socio-ecological studies. I have made no attempt to deal with the problems as such in anything like an appropriate manner, for this would demand an almost encyclopaedic knowledge, which no single person can ever hope to acquire.

According to Gutkind, the term *social ecology*

> . . . stresses the indivisibility of man's interaction with his environment and the need for extending the hitherto rather narrow field of sociological studies to all these factors which are connected in one way or the other with the habitat of man.

Gutkind feels that the chief danger to modern society is the mechanization of life and the subordination of the individual to an impersonal social system that seems to accompany large-scale organizations, whether they are political, professional, or religious. His answer is that power must be decentralized by the creation of many small communities within which men can express all their various social relationships to one another in a personal, neighborly sort of way. He idealistically suggests that "The emergence of communities in a stateless world is the highest ideal which we can discern at the present."

As a biologist, I was particularly interested in what the author had to say about possible contributions of animal ecology to the study of human behavior and sociology; this topic was dealt with in a section of some four pages. Gutkind apparently relies for his conceptions of animal ecology on a review article published in the 14th edition of the *Encyclopedia Britannica* (1929). He mentions various general parallels, well known to zoologists at least, between animal societies and human societies, but there is no specific mention of any of the not inconsiderable work done on social behavior of animals in this country or in Europe during the last 25 years.

The ideas and opinions expressed in this book are usually couched in general terms, with little or no documentation of any supporting evidence, and they can be taken by the reader for whatever they are worth to him.

N. E. COLLIAS
Biology Department, Illinois College

The Rise and Fall of Maya Civilization, by J. Eric S. Thompson. Univ. of Oklahoma Press, Norman, 1954. xii + 287 pp. Illus. + plates. $5.

SINCE planned experiments in social relationships are next to impossible, one needs reliable descriptions of societies that happen to have developed in the past—especially now, when our own shows signs of quite literally blowing itself up. Thompson's new book gives us an authoritative historical reconstruction of the "most advanced" pre-Columbus culture of the American Indians. The classic Maya culture was one of several contemporary developments in what is now Mexico and Central America. Thompson seeks to explain a specialized manifestation in context, not in a vacuum. The period of rise, florescence, and fall is from an estimated 500 B.C. to A.D. 1540, when the Spanish conquered Yucatan.

This is not the first good book on the ancient Maya, but if only one is to be read, I think this one is the "best." It is meant for thinking people in general. Large masses of facts are merely summarized, having been selected for their bearing on the subject of the title. Emphasis is on interpretations and on finding significances, not on proving the facts. Descriptions are vivid, and Chapter V is historical fiction. There are many subheads as well as an index, a "Synopsis of Maya history," and a reading list, but there are no footnotes. The format is attractive; illustrations are good and well chosen. Specially drawn decorations subtly condition the reader in the field of ancient ideas as revealed in ancient Maya art.

Much of the factual basis is the product of archeology, a discipline that turns out quantities of reports

read only by specialists. Some may wonder if this is worth the cost. This book, which is not dull reading, could not have been written without the large backlog of technical literature that is now available, and the author is an outstanding contributor to this basic foundation. For many years he has been a leader in the division of historical research of Carnegie Institution of Washington, digging out ancient Maya "cities," studying the carved stone monuments, the preconquest hieroglyphic manuscripts, and the postconquest Maya writings and learning from the modern descendants of the producers of these things. No one else controls the whole body of evidence so thoroughly.

With such a background, Thompson is justified in offering generalizations and appraisals that go beyond proved facts, entering the realm of intangibles that are, after all, what one wants most to know about. Opinions that are not shared with other experts are labeled as such, and one may be sure they have been checked against all available evidence.

In brief, he tests Toynbee's theory of history and finds it valid for the Maya. The differentiation of classic Maya culture involved the challenge of a hostile tropical forest to soil-loving peasants with Stone Age tools, no draft animals, and no machines. Specific forms of the culture were in accord with the traits of character of the people. This character, "with its emphasis on moderation, discipline, cooperation, patience, and consideration for others, made possible outstanding achievements in the intellectual field." Thompson does not see these people as priest-ridden but rather as having a feeling of participation when they labored on the great ceremonial structures. Decline came with the acceptance of outside influences leading to secularization and militarism, when

> Religion had lost its predominant place in the culture; warfare introduced to bring man closer to his gods had become the master. The Itza (last free Maya group) had journeyed far through time and space to learn that the end never justifies the means, but is itself warped and shaped by them.

LINTON SATTERTHWAITE
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Mineral Nutrition of Fruit Crops. Deciduous and evergreen tree and small fruits. Norman F. Childers, Ed. Horticultural Publ., Rutgers Univ., New Brunswick, N.J., 1954. 907 pp. Illus. + plates. $10.

THERE is no doubt that this book will be a valuable addition to the reference libraries of research workers, field agricultural specialists, growers, and students in the agronomic and horticultural fields. A distinguished group of horticulturalists and plant physiologists contributed the individual chapters, most of which are devoted to reviews of the nutrition literature of specific fruit and nut trees and commercial berries.

Nutrient requirements and deficiency symptoms are stressed for each specific crop, and a separate chapter, containing a useful table summarizing the data for various tissues of each plant for normal and deficient condition, is included. A lengthy photographic section illustrating the various deficiency symptoms is an important feature. The photography is generally quite good, although it is a pity the plates could not have been in color.

Although the chapters on individual fruits have somewhat similar patterns of organization, marked differences in degree of treatment of individual crops exist. This is partly because of the greater volume of research effort that is expended on the more commercially valuable fruits, but it also reflects a greater zeal on the part of some of the authors. In my opinion the chapters on the peach and on citrus fruits are especially well done. The book affords a convenient method for comparing the state of research on different fruits and will be especially useful to research workers on the less-studied fruits in providing ideas for study. The extensive lists of references will also be useful in this regard.

Commendable chapters on leaf analysis, use of isotopic tracers, chelated metals, and experiment planning are included for the apparent purpose of stimulating interest in the use of these techniques among research workers.

PETER C. DUISBERG
Desert Products Company and
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The Physician and His Practice. Joseph Garland, Ed. Little, Brown, Boston, 1954. xii + 270 pp. $5.

COMPOSED of 19 chapters *The Physician and His Practice* is written by 18 authorities who have had the professional experience needed to give a clear, concise understanding of the numerous requirements of a physician in the private practice of medicine. Cognizance is taken of the size of the community in which the practice is located and the difference of the type of problems that thus arise. The authors elaborate on subjects that embrace the general practitioner and the specialist, explaining their obligations to the patients, the community, medical societies, and their own families.

In addition, the character and personality of the physician and that of his wife are emphasized, especially in relation to their leadership in the community.

Considerable discussion centers around the various opportunities that are open to the young physician, dividing them broadly into medicine, surgery, and psychiatry. The need of adequate training and the setting up of the various speciality boards are explained. The physician's affiliation with hospitals and medical societies to help keep abreast of progress is stressed.

Of very special interest to the physician about to enter private practice today is the chapter on group practice (which is increasing rapidly). It presents some of the assets and liabilities of such a practice and thus stimulates serious thought along this line.

There is also outlined the need of conducting the practice in a businesslike manner. Accentuation is

placed on the way of rendering statements, purchasing equipment, and maintaining records and files that will enable the patient, as well as the physician, to know just what his bills cover. Suggested types of records are presented.

In general, the young physician who has just completed his hospital training and is about to enter private practice will gain the most information from this treatise. Nevertheless, it will also serve as a useful reference to the physician who has been following his profession for a number of years. He can refresh his memory to see whether he is keeping his Hippocratic Oath and also getting the most satisfaction from his profession. Any student who is contemplating the study of medicine may glean an insight into his future life and know whether he is suited for the profession before spending the time and money on a medical education.

Thus, Means sums up the book when he writes: "The main purpose of this book is to serve as a guide to the practice of medicine as it applies to the individual."

E. HERBERT BAUERSFELD
Department of Medicine, George Washington University

Psychology: The Unity of Human Behavior. Timothy J. Gannon. Ginn, Boston–London, 1954. xii + 482 pp. Illus. $4.75.

THE relative youth of psychology as a science is clearly reflected in the differences in the content of current introductory textbooks. Because of honest differences in conviction as well as in an effort to produce a widely used textbook, authors vary widely with respect to such matters as (i) the amount of space devoted to the biological/physiological as compared with psychological bases of behavior, (ii) the degree to which they emphasize philosophical (theoretical) orientation(s) in psychology, and (iii) the degree to which psychology is presented as a pure science or as an approach to the solution of practical problems, both individual and group.

Gannon's textbook begins with a brief but tightly written historical chapter designed to provide the student with a particular philosophic orientation to the study of psychology, namely that of Aristotelian rationalism. The next chapter is devoted to the nervous system which is characterized as the "organ of integration and adjustment." Then follow three chapters on the sense organs and one on perception, and three chapters on "Responses" in which the author discusses reflexes, drives, and emotions. Roughly the last half of the book, entitled "Integration," includes chapters on higher conscious processes, imagery and dreams, learning, memory and attention, the measurement and nature of intelligence, conscious control and lastly, a chapter entitled "Personality."

As compared with other recent introductory textbooks, this one is characterized by relatively more emphasis on the nervous system and the sense organs, less emphasis on learning, the adjustment mechanisms, and the practical applications of psychology. Throughout, the author is generally critical of all dualistic, deterministic, and psychoanalytic approaches to psychological problems. In the last chapter, he concludes that "the failure to maintain the importance of the central unifying principle of personality has created a vacuum at the very center of psychology." In his view, only by affirming "the primacy of human intelligence and the autonomy of human choice" can psychology become a true "science of man."

For instructors who share Gannon's general philosophical orientation, this should be an excellent choice as a textbook. The writing is exceptionally lucid, the illustrations are superior, and physically it is an attractive volume. For readers who do not share the author's orientation, this book constitutes a very readable and effective statement of the position that he represents.

E. LOWELL KELLY
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The Pharmacologic Principles of Medical Practice. John C. Krantz, Jr., and C. Jelleff Carr. Williams & Wilkins, Baltimore, 1954. xxi + 1183 pp. Illus. plates. $12.

IN this third edition of a well-known textbook of pharmacology, the authors have succeeded in compressing an enormous amount of material into a volume of ordinary size and reasonable cost. The book is quite up to date and reasonably adequate for general information, and the historical sketches are excellent.

CARL F. SCHMIDT
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Indians of the Plains. American Museum of Natural History Anthropological Handbook No. 1. Robert H. Lowie. McGraw-Hill, New York, 1954. xiii + 222 pp. Illus. $4.75.

TO the average person throughout the civilized world the picture that comes to mind when the subject of the American Indian is mentioned is that of the Plains warrior, tall, handsome, wearing an eagle-feather war bonnet, and probably mounted on a horse.

During the period of westward expansion, the colorful Plains tribes caught the popular imagination until they virtually became a symbol for American Indians in general.

Yet despite this popular interest, until the appearance of the work under review, there has never existed an adequate book treating the Plains area as an integrated unit. Robert H. Lowie, foremost authority on the Plains, has handled a complex subject in a comprehensive and orderly fashion. He well recognizes that the selection of the Plains region as a culture area is based on rather arbitrary criterions. Six major linguistic stocks are represented, and there is considerable variation in physical type and in customs in general. Historically, the places of origin of the many tribes are widely separated. Many of the best-known tribes entered the Plains from the Eastern Woodlands

relatively recent historic times. There were semi-sedentary groups practicing agriculture and others were nomadic. Nevertheless, certain practices throughout the Plains eventually became common to all tribes, thus justifying treatment of the area as a unit. Foremost of these, listed by Lowie, are

> . . . dependence on the buffalo, residence in skin-covered tipis, use of the horse for the hunt and for transport, the peculiar style of decorative and of pictographic art, the sign language, the ideology of warfare, the Sun Dance and less conspicuous features of supernaturalism.

The basis of this combination of traits may well have been present in the nomadic tribes encountered by Coronado in 1541, as were the traits exhibited by the semisedentary tribes seen on the same expedition. The recent arrivals to the Plains built on this basis, and added the horse. This is not surprising since Plains culture has apparently always had more affinity with the Eastern Woodlands than with the Southwestern and Basin-Plateau areas.

The first volume of the new handbook series of the American Museum of Natural History has set a high standard. It will be ideal both as a textbook and for use by the general reader.

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An Introduction to the Study of Insects. Donald J. Borrer and Dwight M. DeLong. Rinehart, New York, 1954. ix + 1030 pp. Illus. $9.

THIS introduction to insect study is offered as the basis for a beginner's course in college entomology, as a guide for teachers and others interested in the study of insects, and as a textbook for advanced work in insect classification.

In attempting to achieve these ends, the authors devote a chapter each to general information on insects and their habits, to anatomy and physiology, and to development and metamorphosis. These are followed by a chapter on classification, nomenclature, and identification and by one that treats the orders generally. A key is provided for the 26 orders recognized. Each order is then treated in sequence in a separate chapter which includes a key to the families (and sometimes to the subfamilies) occurring in the U.S., concise synopses of the families represented, and a section on collecting and preserving the species peculiar to the order. Numerous illustrations depict the adult and immature stages of various species and the structures used in the keys. A selected list of references to the literature is appended to each chapter. The arthropod relatives of the insects are similarly treated; there are keys to the orders and to most of the families found in the U.S.

The last 200 or more pages of the work contain accounts of the relations of insects to man, control of insects, detailed methods of collecting and preserving insects that supplement those given under the various orders, types of collections for study and display, methods of rearing insects, sources of information on entomology, and so forth. A glossary and an index are provided.

The authors have accomplished their purposes well. The treatment of habits, anatomy, physiology, and metamorphosis, although brief, is up-to-date and sufficient for the beginner. Classification and nomenclature are covered thoroughly. The section on the pronunciation of technical names and terms will have wide appeal. Procedure in collecting and preserving insects is dealt with in such detail that the student will seldom need to consult other sources. I welcome particularly the emphasis throughout the book on the study of the living insect.

It is difficult to find fault with this book. Perhaps the excursion into the field of economic entomology has gone further than necessary in an elementary work. At least, the 10 pages on insecticides could have been condensed without loss of usefulness, but their inclusion does not detract from the general excellence of the book.

CHARLES H. RICHARDSON
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Insect Fact and Folklore. Lucy W. Clausen. Macmillan, New York, 1954. xiv + 194 pp. Illus. $3.50.

THE advance publication in *Natural History* (June, 1954) of illustrated excerpts from Lucy Clausen's *Insect Fact and Folklore* aroused an interest and a sense of anticipation that have proved to be fully justified. The book is a compact store of accurate entomological and anthropological material presented in an unusual way. Writing primarily to introduce the layman to the world of insects—their great economic and medical importance and their amazing interest as creatures in their own right—the author has emphasized their close linkage with man from earliest times and their role in his culture.

The character and range of the book can, perhaps, be best suggested by a sampling of the 15 chapter headings: "Velvet wings—the moths and butterflies"; "Insects in armor—the beetles"; "Doggers of civilization—the flies"; "Insect hypodermics—the bugs"; and "Undercover workers—the termites." The information is presented simply and with a minimum of technical terms. The reader is at once told how to recognize an insect and something of its classification, size, structure, growth, and potential powers of reproduction. The forms chosen for discussion are usually well known. Some are important producers of useful substances such as silk, honey, beeswax, lac, and dyes. Others are exceedingly destructive and dangerous.

For the scientist the most instructive aspect of the book will be the "folklore"—the associations of insects with the myths and customs of peoples of various countries, including our own. Along with the more familiar classical stories he will meet, for example, the Indian legend of how South Wind breathed life into beautifully colored pebbles which took wing to become the first

moths and butterflies, or the Chinese myth of the glow-worm, the spirit of a small boy searching for his lost coppers.

At the end of most chapters there is an interesting miscellaneous section containing proverbs, general statements, and symbols, for example, the use of the mosquito on a Mexican revenue stamp to gain funds to combat malaria. A final chapter on "Progress—science and insects" points up some of the major insect problems and some of the latest methods of attack.

Instructive illustrations by Mrs. Fairservis, drawn with expression and imagination, enrich the text.

A bibliography of over 130 items might well have included *The Songs of Insects* (1948) by George W. Pierce and *Bees* (1950) by Karl von Frisch. There is a good workable index which might profitably have listed the countries and peoples whose folklore and customs are treated. One wonders why the cover design of a book on insects should represent a spider.

Lucy Clausen's novel book should reach and be enjoyed by a wide range of readers.

GLADYS K. McCOSH
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Microbes and You. Stanley E. Wedberg. Macmillan, New York, 1954. ix + 439 pp. Illus. $4.50.

THIS textbook has been prepared for a terminal survey course in microbiology, presumably of one semester. It is addressed to students with little scientific knowledge. The style is informal. In general the descriptions of organisms and of processes are lucid. The book is read easily, is interesting, and will probably hold the attention of students.

The first 10 of the 23 chapters are devoted principally to bacteria. Their morphology, metabolism, culture, identification, classification, and relationships with their environments are well described. The next five chapters deal with water, sewage, air, soil, and food, with reference to the microbes they contain. There is especial emphasis on the pathogens and the means of combatting them. Microbial, particularly bacterial, diseases are considered in the four chapters—"Vectors," "Immunity," "Natural barriers to invading organisms," and "Diagnostic tests for immunity and for infection"; quarantine and theories of disease are also treated. The last chapters deal with molds, yeasts, Rickettsiae, viruses, and blood grouping. The molds and yeasts are considered very briefly as to classification, structure, economic importance, and pathogenicity. The Rickettsiae are covered somewhat more fully. The most common of these, their vectors, the diseases themselves, and protections against them are described. Viruses receive similar treatment. The chapter on blood grouping is conventional.

Throughout, the emphasis is upon the pathogens and their effects upon man. There is brief reference to nitrogen bacteria and to the carbon cycle in the chapter on soils. Autotrophic bacteria are referred to in two sentences. Iron bacteria are mentioned once.

Numerous instances of carelessness in editing and writing suggest haste in the preparation of this textbook. Sentence construction is, in some places, so poor as to make the meaning doubtful; for example, (p. 118) "The purity of the microbial culture is just as important, if not more so, than it is in growing grain crops." A number of words are incorrectly used and defined. Sections concerning animal host structure and reaction are much less well done than those dealing with bacteria. The paragraphs headed "Mechanical and physiological barriers of man," (pp. 302 ff.) contain both incomplete and erroneous statements. In speaking of blood as a protective factor, no mention is made of phagocytes; lymph is described as a fluid lacking cellular elements. In another place erythrocytes are said to have cell walls. It is to be hoped that in a subsequent edition these matters will be given attention, for they mar a book that should be useful.

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Intertidal Invertebrates of the Central California Coast. Rev. of S. F. Light's *Laboratory and Field Text in Invertebrate Zoology.* Ralph I. Smith, Frank A. Pitelka, Donald P. Abbott, and Frances M. Weesner. Univ. of California Press, Berkeley, 1954. xiv + 446 pp. Illus. $5.

THIS book is presented as a faunal manual directed to advanced students in general invertebrate zoology and marine ecology. Although it is based on a limited faunal area, there is sufficient generalization of the material that it is to be expected that a wide tradition developed in connection with its predecessor will be continued in associating this volume with a number of local textbooks and syllabuses. A section on field studies, with specific exercises outlined, furnishes an indication of the particularly successful approach of the course that led to the development of this textbook. The body of the work consists of keys and faunal lists by systematic groups, prefaced by expositions and illustrations of taxonomic characters. Descriptions and glossaries such as are not readily available, particularly for the North American fauna, include some notable examples, among others, in the treatments of sponges, polychaetes, crustaceans, molluscs, bryozoans, and tunicates.

Much original material and considerable extension of distributional information are included. Keys and checklists dealing with the sponges, sea anemones, hydromedusae, ctenophores, polyclads, bryozoans, phoronids, echinoderms, tunicates, polychaetes, opisthobranchs, amphipods, isopods, and pycnogonids present findings that are the result of years of field effort and of extensive fundamental research. Most of this information is not available in print elsewhere. In this same category, but perhaps deserving special mention by the novelty of their inclusion, although their crying need in such works has been long acknowledged, are the keys to the intertidal insects, marine mites, intertidal fishes, and algae. Fifty-four pages of painstakingly annotated bibliography conclude the volume.

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Books Reviewed in SCIENCE

4 February

X-Ray Diffraction Procedures, Harold P. Klug and Leroy E. Alexander (Wiley; Chapman & Hall). Reviewed by R. E. Marsh.

Evolution as a Process, Julian Huxley, A. C. Hardy, and E. B. Ford, Eds. (Allen & Unwin). Reviewed by T. Dobzhansky.

Gas Dynamics of Thin Bodies, F. I. Frankl and E. A. Karpovich (Interscience). Reviewed by C. Truesdell.

Protein Metabolism, R. B. Fisher (Methuen; Wiley). Reviewed by C. S. Vestling.

Structure Reports for 1950, vol. 13, A. J. C. Wilson, Gen. Ed. (Oosthoek). Reviewed by G. W. Brindley.

Comparative Anatomy of the Vertebrates, George C. Kent, Jr. (Blakiston). Reviewed by A. S. Romer.

Reviews of Research on Problems of Utilization of Saline Water, Arid Zone Programme, No. IV, (UNESCO) (Columbia Univ. Press). Reviewed by S. T. Powell.

11 February

Optical Image Evaluation, Proc. of NBS Symposium held 18–20 Oct. 1951 (Supt. of Documents, GPO). Reviewed by G. Westheimer.

Plant Regulators in Agriculture, H. B. Tukey, Ed. (Wiley; Chapman & Hall). Reviewed by J. C. Frazier.

Thermo-Mikro-Methoden, Ludwig Kofler and Adelheid Kofler (Verlag Chemie). Reviewed by G. B. Bachman.

Biochemical Determinants of Microbial Diseases, Rene J. Dubos (Harvard Univ. Press). Reviewed by R. D. Housewright.

Differential Equations with Application, Herman Betz, Paul B. Burcham, and George M. Ewing (Harper). Reviewed by N. J. Rose.

Psychomotor Aspects of Mental Disease, H. E. King (Harvard Univ. Press). Reviewed by R. C. Davis.

The Elementary Chemical Composition of Marine Organisms, Mem. No. 2, A. P. Vinagradov (Yale Univ. Press). Reviewed by V. T. Bowen.

An Introduction to Human Biochemical Genetics, H. Harris (Cambridge Univ. Press). Reviewed by B. Childs.

Qualitative Inorganic Analysis, G. Charlot (Methuen; Wiley). Reviewed by F. E. Brown.

General Theory of High Speed Aerodynamics, vol. VI. *High Speed Aerodynamics and Jet Propulsion,* W. R. Sears, Ed. (Princeton Univ. Press). Reviewed by C. B. Millikan.

25 February

Neuere Probleme der Abstammungslehre-Die transspezifische Evolution, Bernard Rensch (Ferdinand Enke Verlag). Reviewed by A. A. Buzzati-Traverso.

The Mind and the Eye, Agnes Arber (Cambridge Univ. Press). Reviewed by W. R. Amberson.

Relative Chronologies in Old World Archaeology, Robert W. Ehrich, Ed. (Univ. of Chicago Press). Reviewed by M. J. Mellink.

Biochemistry and Human Metabolism, Burnham S. Walker, William C. Boyd, and Isaac Asimov (Williams & Wilkins). Reviewed by W. Frajola.

The Theory of Metals, A. H. Wilson (Cambridge Univ. Press). Reviewed by K. Lark-Horovitz.

Abhandlungen aus der Sowjetischen Astronomie, Folge II. Gesellschaft fur Deutsch-Sowjetische Freundschaft, Otto Singer, Ed. (Verlag Kultur und Fortschritt). Reviewed by D. ter Haar.

The Manual of Antibiotics, 1954–1955, Henry Welch, Ed. (American Pharmaceutical Assoc.). Reviewed by R. E. Crist.

Pigment Cell Growth, Myron Gordon, Ed. (Academic Press). Reviewed by G. DuShane.

Explaining the Atom, Selig Hecht. Rev. by Eugene Rabinowitch (Viking). Reviewed by O. Oldenberg.

Linear Transient Analysis, vol. I. *Lumped-Parameter Two-Terminal Networks,* Ernst Weber (Wiley; Chapman & Hall). Reviewed by L. D. Runkle.

Connective Tissues, Trans. of the 4th Conference, 18–20 Feb. 1953, Charles Ragan, Ed. (Josiah Macy, Jr., Fdn.). Reviewed by J. A. Arcadi.

Grundlagen und Praxis chemischer Tumorbehandlung. Zweites Freiburger Symposion an der Medizinischen Universitäts-Klinik. J. Pirwitz (Springer). Reviewed by S. Peller.

New Books

Los Hidro-Metabolitos y las "Enfermedades de la Civilizacion." P. Puig. Libreria Editorial, Argos, S.A., Barcelona Buenos Aires, 1954. 325 pp. Paper, P. 100.

Fundamentals of Electrical Engineering. Based on the rationalized M.K.S. system of units. Edward Hughes. Longmans, Green, London–New York, 1954. 470 pp. $2.50.

Modern Chemical Discoveries. Richard Clements. Dutton, New York, 1954. 290 pp. $5.

Reports of the Biochemical Research Foundation of the Franklin Institute, vol. XII, 1952–1953. With an index to BRF pubs. Nos. 1 to 263 and BRF notes Nov. 1938 to Dec. 1953, bound in reports from BRF, vols. I to XII, 1930–1953. Biochemical Research Foundation, Newark, Del., 1954.

Needed Research in Health and Medical Care. A biosocial approach. Cecil G. Sheps and Eugene E. Taylor. Univ. of North Carolina Press, Chapel Hill, 1954. 216 pp. $5.

Practical Clinical Biochemistry. Harold Varley. Interscience, New York; Heinemann, London, 1954. 551 pp. $6.50.

The New Men. A novel. C. P. Snow. Scribner's, New York, 1954. 311 pp. $3.50.

Index XII to the Literature of American Economic Entomology, 1952. Compiled by Ina L. Hawes. Entomological Society of America, Washington 5, 1954. 321 pp. $3.

Studies in Mathematics and Mechanics. Presented to Richard von Mises by friends, colleagues, and pupils. Academic Press, New York, 1954. 353 pp. $9.

A Bibliographical History of the Ray Society. Richard Curle. Ray Society, London, 1954. 101 pp. £10 6s.

Our American Weather. George H. T. Kimble. McGraw-Hill, New York–London, 1955. 322 pp. $4.75.

Probleme und Beispiele biologischer Regelung. R. Wagner. Georg Thieme, Stuttgart, 1954. 219 pp. DM. 29.40.

Scientific Books, Libraries and Collectors. A study of bibliography and the book trade in relation to science. John L. Thornton and R. I. J. Tully. The Library Association, London, 1954. 288 pp. 24s.

High-Energy Accelerators. M. Stanley Livingston. Interscience Tracts on Physics and Astronomy, No. 2, R. E. Marshak, Ed. Interscience, New York–London, 1954. 157 pp. $3.25.

The Plant Quarantine Problem. A general review of the biological, legal, administrative and public relations of plant quarantines with special reference to the U.S. situation. W. A. McCubbin. vol. XI of *Annales Cryptogamici et Phytopathologici.* Frans Verdoorn, Ed. Ejnar Munksgaard, Copenhagen, 1954 (U.S. Distrib.: Chronica Botanica, Waltham ,Mass.). 255 pp. $4.80.

Einfuhrung in die biologische Registriertechnik. Herbert Klensch. Georg Thieme, Stuttgart, 1954. 222 pp. DM. 33.

Engineering Cybernetics. H. S. Tsien. McGraw-Hill, New York–London, 1954. 289 pp. $6.50.

Dictionary of Last Words. Compiled by Edward S. Le Compte. Philosophical Library, New York, 1954. 267 pp. $5.

Composition of Scientific Words. A manual of methods and a lexicon of materials for the practice of logotechnics. Roland Wilbur Brown. Roland W. Brown, Natl. Museum, Washington 25, 1954. 882 pp. $8.

Standard Values in Nutrition and Metabolism. Being the second fascicle of a Handbook of Biological Data. Prepared under direction of Committee on the Handbook of Biological Data, American Institute of Biological Sciences, Natl. Research Council. Errett C. Albritton, Ed. Saunders, Philadelphia–London, 1954 380 pp. Paper, $6.50.

The Origin of Russia. Henryk Paszkiewicz. Philosophical Library, New York, 1954. 556 pp. $10.

Diagnostic Cytologique du Cancer Génital chez la Femme. Raymond Bourg, Claude Gompel, Jean-Paul Pundel. Masson & Cie, Paris, 1954. 176 pp. F. 4.800

Contributions to Embryology. vol. 35, Nos. 231–241. Carnegie Institution of Washington, Washington 5, 1954. 237 pp. Paper, $12; cloth, $13.

An Essay on Christian Philosophy. Jacques Maritain. Trans. by Edward H. Flannery. Philosophical Library, New York, 1955. 116 pp. $2.75.

The Bomb, Survival and You. Protection for people, buildings, equipment. Fred N. Severud and Anthony F. Merrill. Reinhold, New York, 1954. 264 pp. $5.95.

Introduction to Theoretical Mechanics. Robert A. Becker. *International Series in Pure and Applied Physics.* Leonard I. Schiff, Ed. McGraw-Hill, New York–London, 1954 420 pp. $8.

Glutathione. Proc. of the symposium held at Ridgefield, Connecticut, Nov., 1953. S. Colowick, A. Lazarow, E. Racker, D. R. Schwarz, E. Stadtman, and H. Waelsch, Eds. Academic Press. New York, 1954. 341 pp. $7.50.

Tobati: Paraguayan Town. Elman R. Service and Helen S. Service. Univ. of Chicago Press, Chicago, 1954. 337 pp. $7.

The Scientific Revolution, 1500–1800. The formation of the modern scientific attitude. A. R. Hall. Longmans, Green, London–New York, 1954. 390 pp. $3.50.

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(*Continued on page viii*)

LETTERS

Variations in Productivity among Creative Workers

In 1910 James McKeen Cattell wrote (*1*): "We do not know whether progress [in science] is in the main due to a large number of faithful workers or the genius of the few." Today the general problem posed by Cattell is still unsolved. Without trying to assess "progress" in science or elsewhere, I shall, however, attempt to deal with Cattell's problem as it relates to one tangible variable in creative work, namely, productivity as defined in terms of sheer number of works produced (*2*).

The areas analyzed were chosen on a twofold basis: (i) they represented diverse fields; and (ii) a convenient bibliography, relatively complete for some specified period of time, was available in each field. These areas and the sources of data were as follows:

1) A bibliography of secular music published in America during the 18th century (*3*).

2) The books represented by the Library of Congress printed cards, as of 1942 (*4*).

3) A recently published bibliography of gerontology and geriatrics (*5*).

4) A bibliography of North American geology for 1929–39 (*6*).

5) A bibliography of infantile paralysis, 1789–1944 (*7*).

6) The decennial index to *Chemical Abstracts* for 1937–46 (*8*).

7) A bibliography of linguistics for 1939–47 (*9*).

It should be noted that each of these bibliographies involves a minimum of selectivity and of evaluation. Practically all published materials coming within the scope of any of these bibliographies may be presumed to have been included.

Within each bibliography, a sample of 200 authors was chosen for analysis. Insofar as possible these samples were chosen to obtain a random selection. The method of sampling varied somewhat with the nature of the bibliography and the arrangement of the author index. Wherever feasible, the procedure was to determine what proportion *n* of the total number of authors in a bibliography should be chosen to obtain a sample of 200, and then to choose every *n*th name in alphabetic order. However, in order to simplify the task, in some instances certain names on every *x*th page or column were taken instead of every *n*th name. Thus, in Shock's bibliography (*5*) we turned to the name index and chose the name at the top of each column, a procedure that yielded approximately 200 names. To obtain the round number of 200 in each sample, a procedure that would have yielded slightly more than 200 was stopped after 200 names had been obtained. If slightly fewer than 200 names were obtained, the necessary additional names were picked at random.

After the authors had been selected, the total number of items attributed to each man in the bibliographic source was recorded. The men of a given group were arranged in order according to their degree of productivity. The total productivity of the entire 200 was then determined. Finally, the proportion of the total produced by each decile was calculated. The results of this analysis are shown in Table 1. In addition, Table 1 shows the total number of items produced by each sample of 200 and also the number of items produced by the top-ranking person in each group. The right-hand column indicates how many persons in each group contributed only one item each to the bibliography.

Several aspects of the data presented in the table should be noted. Among these are the high proportions of the total number of items contributed by the first deciles, ranging from 64 to 34 percent of the total number of items in various populations. The median value for the contribution of the top decile is 49 percent. In other words, in each of several creative fields, 10 percent of the men perform roughly 50 percent of the work. The contributions of deciles beyond the first decile become progressively less until a plateau is reached, the plateau being due to the large number of persons who produce only one item each.

If the three highest deciles are combined, it will be found that their share of the group productivity ranges from 85 to 59 percent of the total. If the men in the bot-

Table 1. Distribution of productivity among creative workers.

Group	Total items	Highest individual record	Proportion of total produced by each decile										No. of persons in 200 producing only one item each
			1	2	3	4	5	6	7	8	9	10	
Music	1351	146	0.64	0.14	0.07	0.04	0.03	0.02	0.01	0.01	0.01	0.01	91
Books	621	56	.53	.13	.08	.06	.03	.03	.03	.03	.03	.03	120
Gerontology and geriatrics	572	47	.49	.13	.09	.07	.04	.03	.03	.03	.03	.03	116
Geology	837	64	.49	.18	.10	.06	.05	.02	.02	.02	.02	.02	101
Infantile paralysis	401	35	.46	.12	.08	.05	.05	.05	.05	.05	.05	.05	149
Chemistry	479	34	.45	.15	.09	.06	.04	.04	.04	.04	.04	.04	131
Linguistics	346	16	.34	.14	.11	.06	.06	.06	.06	.06	.06	.06	142

tom 50 percent of each group are combined, their total contribution is, in every instance, less then the contribution of the highest decile.

In certain respects the productivity of the less active persons is overestimated by our figures. In our data only those members who produced *something* were included. A man may be a professional chemist, geologist, linguist, and so on, and publish *nothing* within a specified period of time. If representation in our lists had been based on professional affiliation instead of on listings in a bibliography, the proportionate contribution of the less productive components of a group would have been even less than has been indicated.

An examination of the record of the highest ranking man in each group shows that in one group, music, this man was the author of 10 percent of the total number of items. In several other groups the contribution of the most productive individual was almost as high. We refer, of course, to the relationship of his contribution to that of a sample of 200 and not to the relationship of his contribution to the total works in his field. However it should be noted that, in absolute terms the highest record among all persons in a field will usually be considerably larger than the record of the most productive member of a random sample of 200.

It is interesting to compare the output of the most productive person in a group of 200 with the output of men toward the bottom of the group, most of whom were responsible for only one item. Thus the output of the highest ranking man in the music group was equal to the total output of the 100 men who were below the median of the group. The most productive author in the books group had an output equal to the combined output of the 56 least productive authors. In geology, it would be necessary to add to the group 64 low-decile men to counterbalance the loss of the single most productive geologist found in our sample. Relationships within the other groups, which can be calculated from Table 1, are comparable to those just cited.

The figures to which I have just called attention show clearly that the majority of creative contributions is made by a minority of the contributors, and that the numerous low-productivity persons are responsible for a very small part of the total. These data for seven diverse fields of creative work are in essential agreement with the finding previously published for psychologists (*10*).

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Dogmatism as an Element of Acceptance of Theory

The articles in the symposium *Reasons for the Acceptance of Scientific Theories* [*Sci Monthly* **79**, 139 (1954)], particularly the one by Philipp G. Frank, seem to lose themselves in abstract involved logic. Apparently the difficulty is a confusion of scientific theories that have an objective basis (here, the principal characteristic is an absence of dogmatism) and theories that have a subjective basis (here, dogmatism is the heart of belief). Thus, the conception that one must worship according to a certain ritual and have certain beliefs to stand in with God is only a postulate; the same applies to many social and economic beliefs. Yet many people are dogmatic to a point where they are willing to kill those who disagree with their religious, social, or economic beliefs. Many followers of Karl Marx are an example of the last, although it is improbable that the theories of a man who lived more than 100 years ago and who had little industrial experience, could have any validity as applied to our modern managerial (as distinct from capitalistic) industrial system. Many anti-Marxists are almost as dogmatic.

Frank treats the Copernican-Ptolemaic controversy as a conflict between two scientific theories, regarding both as if they had nondogmatic adherents. As a matter of fact, the Ptolemaic theory became part of religion and had such an emotional basis that it was dangerous, as Galileo found, to express a belief that Copernicus may have been right.

In fact, while tolerance is the very heart of scientific theory, the presence of tolerance can destroy many emotionally based theories. An orthodox Jew and a Catholic hold conflicting religious theories, but if both are tolerant, each must say to the other, "You may believe what you want and I will believe what I want." Hence, in an important matter such as religion—and lately religion has been given the utmost importance—everyone, to be tolerant, believes what he wants to believe. Since an approach applicable to religion must certainly be applicable to less-important social institutions, we find ourselves in a position to believe what we want to believe in our everyday dealings. This, I submit, leads to a highly immoral social system.

I realize that there is no sharp line between emotional and scientific theories. A scientist may have an emotional reason for adhering to a scientific theory. But scientists may have learned, as lawyers have, that sharp categories are few or nonexistent, and we must recognize differences of degree and proximity. In the limit a circle becomes merged into a straight line, but as a rule we know when we are dealing with circles and when we are dealing with straight lines.

HYMEN DIAMOND

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Hymen Diamond objects to the "abstract involved logic" that is used in the papers in the symposium on *Reasons for the Acceptance of Scientific Theories,* and especially in my paper. The purpose of my paper was to state the reasons for the acceptance of theories in a way that is in itself scientific and is applicable to all kinds of theories. Diamond restricted himself to a very oversimplified formulation of the criterions for acceptance by simply distinguishing between theories with an objective basis and theories with a subjective basis. This is a very misleading distinction. The basis of all theories is the data of our experience; in this sense, they all have an objective basis. However, every theory attempts to build up a set of principles by means of our creative imagination and to derive the facts of our experience from a small number of invented principles. In this sense, every theory also has a subjective element in it. We can say that every theory has an objective basis and a subjective top.

The term *dogmatic* does not refer to a theory; it refers to the social role of a theory; it refers to the fact that the acceptance of a theory is enforced by or not enforced by some authority. The views of the orthodox Catholic are not more subjective than the views of the engineer. The philosopher of the Middle Ages believed that he could prove the existence of God as objectively as one can prove the existence of gravitation or any other generalization. The question is merely which theory is more practical in obtaining a certain purpose. But this question can be answered only if we describe the purpose that we want to achieve. The choice of a particular purpose is, in a certain sense, subjective, but, in a broader sense, it is objective too, because the choice depends upon the social situation of the scientist. From this situation, it can be predicted, to a certain extent, whether one will prefer a theory that is technologically helpful or a theory that can exert a direct influence on human behavior, for instance, a moral or religious influence.

Diamond objects to treating the Copernican-Ptolemaic controversy as a conflict between two scientific theories. He thinks that the Ptolemaic theory had only dogmatic adherence. This is certainly not correct. It is well known that men like Francis Bacon and Tycho Brahe, both men of scientific minds, rejected the Copernican theory on scientific grounds. Even Newton spoke of the Ptolemaic theory with high regard. The decision between these theories could become final only when a historical situation developed that gave more importance to the mathematical simplicity of a theory than it did to its helpfulness in supporting the doctrine of traditional religion. I do not think that an orthodox Jew and an orthodox Catholic would say to each other, "You may believe what you want, and I will believe what I want." If people could speak in this way, they would not be orthodox. Thomistic philosophy claims that its arguments make no appeal to emotion but are completely rational. It does not make any sense to say simply that a circle consists of a straight line or that it does not consist of a straight line. It depends upon the problem to be solved by this statement. Each statement is true or not true in relation to the problem to be solved.

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Signals through Space

Two errors in my article, "Signals through space" [*Sci. Monthly* **79**, 170 (Sept. 1954)] have been called to my attention. Charles A. Federer, Jr., editor of *Sky and Telescope*, noticed that Fig. 11 (p. 175) was not a photograph of the Large Magellanic Cloud, but a photograph of one part of the Andromeda nebula.

Walter Baade of Mount Wilson and Palomar Observatories pointed out that "the final distance of the Andromeda nebula will be in the neighborhood of 2 million light years," not 750,000 lt yr (p. 173).

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The Large Magellanic Cloud [Harvard College Observatory]

Food for Mankind

S. A. Asdell [*Sci. Monthly* **79**, 318 (1954)] is certainly correct in his discussion of food-raising when he points out the trend toward rapid growth at the expense of flavor. It may be all too true that the meat of the future will be cultured in hay infusions and served in thin wafers.

Since farmers may not be able to grow food enough for the increasing population, our food scientists might find it worth while to expand their research on nonbiological sources. William C. Rose, of the University of Illinois, has shown that eight relatively simple amino acids—which can be ingested as such—will supply the total nitrogen requirements of the human body. All eight of these acids have been synthesized.

In the United States we put onto the soil each year about twice as much fixed nitrogen as we consume in our food. About 85 percent of this nitrogen is fixed by means of energy (or materials) obtained from oil, coal, or natural gas.

It would be fairly easy to use the nitrogen in chemical reactors and synthesize a large part of our amino acids directly. Then our farmers could concentrate on growing things that taste good. We could feed more people, and all of them could eat like epicures!

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ASSOCIATION AFFAIRS

AAAS SOCIO-PSYCHOLOGICAL PRIZE

Through the generosity of an anonymous donor, the AAAS offers an annual prize of $1000 for a meritorious essay in socio-psychological inquiry. The conditions of competition for the prize to be awarded at the 1955 annual meeting, Atlanta, Georgia, 26–31 December, are as follows:

1) The contribution should further the comprehension of the psychological-social-cultural behavior of human beings—the relationships of these hyphenated words being an essential part of the inquiry. Whether the contributor considers himself to be an anthropologist, a psychologist, a sociologist, or a member of some other group is unimportant, as long as his essay deals with basic observation and construction in the area variously known as social process, group behavior, or interpersonal behavior. For ease of reference in the rest of this statement, this general area will be called *social behavior*.

2) The prize is offered to encourage studies and analyses of social behavior based on explicitly stated assumptions or postulates, which lead to testable conclusions or deductions. In other words, it is a prize intended to encourage in social inquiry the development and application of dependable methodology analogous to the methods that have proved so fruitful in the natural sciences. This is not to state that the methods of any of the natural sciences are to be transferred without change to the study of social behavior, but rather that the development of a science of social behavior is fostered through observation guided by explicit postulates, which in turn are firmly grounded on prior observations. It may be taken for granted that such postulates will include a spatial-temporal framework for the inquiry. It may properly be added that the essay should foster liberation from philosophic-academic conventions and from dogmatic boundaries between different disciplines.

3) Hitherto unpublished manuscripts are eligible, as are manuscripts that have been published since 1 January 1954. Entries may be of any length, but each should present a completed analysis of a problem, the relevant data, and an interpretation of the data in terms of the postulates with which the study began. Preference will be given to manuscripts not over 50,000 words in length. Entries may be submitted by the author himself or by another person on his behalf.

4) Entries will be judged by a committee of three persons considered well qualified to judge material in this field. The judges will be selected by a management committee consisting of the vice president and secretary of Section K and the administrative secretary of AAAS. The Committee of Judges reserves the right to withhold the prize if no worthy essay is submitted.

5) Entries should be sent to Dael Wolfle, Administrative Secretary, American Association for the Advancement of Science, 1025 Connecticut Avenue, NW, Washington 6, D.C. Entries should be submitted in quadruplicate. The name of the author should not appear anywhere on the entry itself but should be enclosed on a separate sheet of paper which also gives the author's address and the title of his essay. To be eligible for consideration for the prize that will be awarded at the 1955 annual meeting of the Association, entries must be received *not later than 1 September 1955*.